# THE PROCEEDINGS OF THE PHYSICAL SOCIETY

VOL. 48, PART 6 November 1, 1936 No. 269

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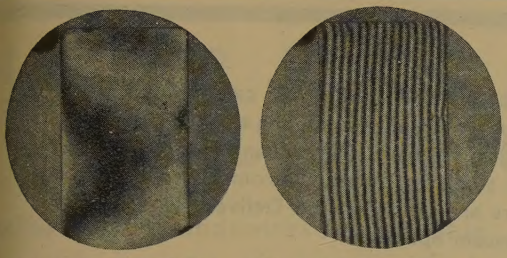

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# THE PROCEEDINGS OF THE PHYSICAL SOCIETY

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530.1

## PHYSICAL ULTIMATES

BY PROF. F. A. LINDEMANN, M.A., PH.D., F.R.S.

*The twenty-first Guthrie Lecture, delivered on May* 15, 1936

I WILL commence by telling you a little fable.

### ERRATUM

"The density and coefficient of expansion of liquid gallium over a wide range of temperature", by W. H. HOATHER, M.Sc., A.Inst.P., *Proc. phys. Soc.* **48**, 699 (1936).

In the footnote on p. 705

*for* page 704 *read* page 703.

particular. On the other hand, he was assured that if he integrated round a contour roughly 2 in. broad and 1 in. deep round each dot, he would be pretty safe in assuming these areas contained 1 unit.

"Why", he asked, "must I do this?"

"Obviously", was the reply, "to take account of the boundary conditions."

"Why is it", he asked, "that the dots so seldom are close together?"

"Clearly", was the reply, "because of the exclusion principle."

"But here", he pointed out, "are two of them quite close together."

"Ah, but these", he was told, "obviously have opposite spins."

"At any rate", he said, "if I promise to observe all these rules, can I rely upon your map?"

"Well, not exactly," was the reply, "for however accurately we make our observations, by the time we have drawn the map and go back to check it up, we always find the dots are in the wrong places."

"What is the use of that sort of map to me?" he said.

"Well", said the cartographers, "we have made whole bookfulls of maps and by careful study we have found a correlation between the probable place of the dots and the right ascension and declination of the sun and all sorts of other rules such as the probability of finding a large number congregated together when one dot disappears or when two coalesce. We cannot of course guarantee anything but if you apply these rules you will find that you have a pretty good chance of predicting the right result."

"But what is it all about?" asked our earnest enquirer, "and why have we to give up our old, simple conventions which I could understand and which worked so well?"

The cartographers looked at him with shocked disapproval.

"What on earth is the good of asking a ridiculous question like that? We have given you a set of rules which will tell you anything that can be told and all that any respectable enquirer ought to wish to know."

So our student was thrown back upon his own resources. He took his maps home and pondered over them and finally came to the conclusion that a human being could not be subdivided and that the boundary conditions merely represented the fact that his projection in plan covered some 2 ft. by 1; that the exclusion principle arose from the fact that no two could occupy the same space, though occasionally, if their spins were of opposite sign one sat upon the other's knee. That they moved about and had definite habits such as going to the City in the day-time and home to bed at night and that they tended to congregate at funerals and marriages. But when he went and told the cartographers this they told him to go to a place where all maps were printed on asbestos paper.

I confess I am in great sympathy with our earnest enquirer in the fable. For something very similar has recently happened to many experimental physicists. Notoriously the methods of classical physics have proved inadequate. In their place we have been given various forms of quantum dynamics, matrices and $q$ numbers, $\psi$ functions and operators. All of them are elegant, original and what is more they deliver the goods. But none of them seem to claim or even wish to claim any physical basis. They are admirable, perhaps perfect rules for calculating what we shall observe, but the ordinary experimental physicist asks what has gone wrong with our classical way of looking at things. Why have we had to introduce these new forms of mathematics, what does it all mean? I will endeavour to-night to explain my view of what has happened. To do this it is necessary to go right back to the metaphysical basis of scientific research and to discuss the ultimate physical concepts or indefinables in terms of which our theories are expressed.

Philosophers seldom agree. But if there is one point upon which they are unanimous, it is that the only absolute knowledge we have or ever can have, is the knowledge of our own sense experiences. A consensus of opinion in such quarters is impressive. All of us, I imagine, will be prepared to accept the sense data as our raw material.

The task of the physicist is to codify these sense data and to work them into a coherent system.

The solipsist's position, that there is nothing beyond ourselves, that all our experiences are merely imaginary, that is to say conditioned by no external stimuli, is a tenable and indeed incontrovertible one. But the physicist is bound to reject it.

For a solipsist has no need of physics. Conversely no physicist can be a solipsist. As we have said, the solipsist's position is admittedly impregnable. So is that of the bankrupt. The physicist refuses to glory in mental insolvency.

The physicist therefore assumes, and nobody can do more than assume it, that there is something external to himself, an external world between which and his sense experiences there is some relation. His aim is to make some picture, model or map of this external universe.

It is often said that his job is merely to predict what is going to happen. This is not true. A tide machine can predict the tides. We demand more than this. We desire to relate them to something else, to fit them into our mental picture. After all, any set of recurrent events can in principle be analysed out of its context by the method of Fourier series. If we were content merely with prediction, this would be all that was required. But we are not. We have progressed from the Babylonian Saros cycle to the system of Ptolemy, from Ptolemy to Copernicus, from Copernicus to Kepler, from Kepler to Newton and from Newton to Einstein.

Man has never been content merely with noting recurrences. He wants a system, he desires to "understand"; and it is this instinct which the physicist endeavours to satisfy.

The physicist then is somewhat in the position of a man who is called upon to make a map of the globe. A certain amount of knowledge is available from past experiences. He can, and if he is an experimentalist, he should explore fresh regions himself. What his present state of knowledge about the universe is, it is difficult to estimate. As regards the physical sciences, I would suggest that it compares with that of the average yokel in Italy in the year 500 B.C. regarding the geography of the globe.

Now it is evident that whatever map the physicist makes will never be complete. No map ever can be. If it were it would cease to be a map and simply be a duplicate of the whole external world. The idea of a map, picture or model implies a certain selection. It only pretends to represent certain aspects. But between these aspects and our sense experiences we are entitled to demand a one-to-one relation.

The sense data with which the physicist is mostly concerned are the result of experiments designed for a particular purpose. A difficulty of selection in some ways analogous to that mentioned above confronts us here also. For no experiment can be exactly repeated, and some way of deciding which circumstances matter in the arrangement of the experiment has to be found. When we observe that a certain object placed twice running in the pan of a spring-balance causes it each time to descend a distance equal to a given ruler, it is quite arbitrary to state that the experiment has been repeated. In the interval the plants have grown, the observer has aged, some thousand million people have moved, the temperature has changed, electric charges have been displaced and the planets and stars have altered their positions. What warrant have we for neglecting all this?

The justification, such as it is, is instinctive. However often we carry out the experiment we observe the same result. Once, twice, or three times, one might believe that external effects might cancel one another out, but not an indefinite number of times.

How many times, it may be asked, must an experimental result repeat before we are justified in refusing to believe they have by chance neutralized one another and in coming to the conclusion that the varying external circumstances have no influence upon the observation? There is no answer to this *a priori*. It is a matter of instinct, of taste. If someone refuses to accept your estimate you can never prove him wrong; the only thing to do is to ignore him.

This instinctive criterion of course only applies to events which repeat. Many experiences, for instance those most important in our own personal history, show no tendency to recurrence. As regards these, therefore, we cannot apply the instinctive test described above. We can only appeal to "physical theory". So far as any criterion goes, the position of the stars and planets at any given moment might perfectly well influence the course of our individual lives; a claim which astrologers have maintained for millennia, not without pecuniary profit.

Thus we see that there is another justification for the physicist's endeavour to form a coherent map or model of the external universe. Not only do we derive aesthetic pleasure therefrom; it enables us to estimate what circumstances are likely to affect observations, or, at any rate, we believe it does. Most of us think this justifies us in spending our lives in trying to draw such a map or helping others to fill in its details.

We now come to the fundamental question, how shall we draw our map? What conventions shall we adopt, what co-ordinates shall we use?

In ordinary map making there are dozens of different systems, all in certain circumstances justifiable. We have one-dimensional maps like the R.A.C. route maps; ordinary two-dimensional maps with contour lines and colours, etc. indicating heights; three-dimensional "relief maps". There are maps whose contour lines indicate rainfall, density of population or geological formation, maps on Mercator's projection and on a host of other schemes. All or any of them can be justified provided there is a one-to-one relation between the map and what it is intended to represent and provided the particular convention used is clearly stated and understood.

The ordinary conventions of cartography by no means exhaust the possibilities; a map or picture is by no means the last word. A long table of figures for instance might be made to serve the same purpose. Thus for instance each row of figures could represent a small area; the first group could give the latitude, the second the longitude, the third the height above sea-level, the fourth density of population, the fifth rainfall, etc. Such a table of figures would have many advantages. An unlimited number of factors could be taken into account. The distance between any two points could be derived by a simple calculation from the first two groups of figures. The deviation from the Pythagorean formula would be a measure of the curvature of the globe. The rainfall or the population of any given region could be discovered by a simple integration. All the facts could be represented by a figure in a polydimensional space. If one were prepared to multiply one's dimensions they could all be contracted to one point in a space of sufficient complexity and the history of the whole world represented by one world line. But there would be one grave dis-

advantage. A calculation would always be required before the content of the map could be apprehended.

Despite the immense freedom and latitude all these methods of representation provide, there are certain circumstances in which they break down. One instance was described in my fable. It would be easy to name many others. But we are concerned to-night with physics.

Whilst the cartographer has at his disposal an almost unlimited set of possibilities and is allowed and indeed expected to use a different map for each quality which he is endeavouring to represent, the physicist is expected to portray everything upon the same map. This makes his task much harder. *Sub silentio*, of course, many of us do use different maps, as when we are thinking for certain purposes in terms of Euclid or Galileo rather than of Einstein. But we do not boast of doing this and indeed many of us refuse to admit it. If we have to use the same map to represent all aspects, of course, the choice of co-ordinates and conventions becomes much more difficult and important; difficult for obvious reasons; important because if one only has one map, one is apt to forget that it is merely a map and to mistake it for reality. In principle, of course, as we have said, the choice is a matter of taste. All that one can insist on is that a convention be not ambiguous and that it be clearly set forth. But the word "taste" implies a great deal. It implies that the choice commend itself to those concerned to deal with the matter, i.e. that the map be intelligible to those who have to use it. It implies by the same token, simplicity, i.e. that it involve a minimum of mental effort to understand. A book is required not a cryptogram, a dictionary not a cross-word puzzle. But even a book is no use unless we understand the language in which it is written. Some of us find some languages so hard to understand that we can only spell out the meaning of the beautiful poems which have been written in these languages and miss almost all the joy and pleasure which the authors so evidently take in them.

My purpose to-night is to plead for a translation into the vulgar tongue. I am quite convinced that the main underlying fundamental metaphysical basis of the modern quantum dynamics can be made intelligible to the average experimental physicist. I urge that this should be done. After all, very few read the Scriptures in the original Greek, even in Oxford. Far fewer no doubt than pretend they do. Most of us are prepared to sacrifice some of the lapidary clarity which we are told distinguishes the classical tongues for a sound comprehension of the language. There was a good deal to be said for the production of the Authorised Version. Can we not have one in physics?

Since none has been forthcoming from the mathematicians I venture to put a sketch before you to-night which does, I believe, explain in physical terms what is implied by the new forms of theoretical physics. The difficulties have arisen, I am confident, because we are using as our ultimate concepts indefinables which are not really appropriate to the external world we are endeavouring to describe. To make my view clear I must consider the fundamental concepts which are in current use.

In Victorian times the particular set of mental co-ordinates or physical concepts, which should be employed in our description of a physical universe, was not con-

sidered to be a matter of doubt. Space and time were taken for granted as ultimate indefinables as they had been since the time of the Greeks. The concept of mass ever since Galileo and Newton had been the third indefinable which enabled one to make a coherent picture of mechanical processes. In the course of the nineteenth century various new derived concepts were added, electric charge and potential, magnetic dipole and field and so forth. But all of these could be defined in mechanical measure once the laws governing electro-magnetic interactions were formulated. One group of concepts did not fall into line in this respect, namely those derived from the very elementary notion of temperature such as the entropy. It was only Boltzmann's brilliant and original interpretation of the entropy in terms of the kinetic theory which made this intelligible.

Many of the derived concepts of mechanics gained acceptance because their effect was so easily perceptible or because they had such primitive analogues. Our muscular reactions make us believe that we know what a force or a pull is; experience in the nursery teaches us to appreciate the idea of an impact with its correlated concept of momentum. Some derived quantities such as energy took many years to become popular. It is strange when one reads older scientific papers to realize how difficult it was for this particular concept to achieve universal understanding and recognition. Habit and use has to-day made most physicists believe that it is as familiar to them as the idea of mass or even length or time; an instructive instance of how easily it is possible by habit and use to gain the impression that one of these physical concepts is a thing we can apprehend *a priori*.

A concept of even more general application than energy is that of action. This unhappily has not penetrated our consciousness with the same success as the energy or momentum concepts. Its many definitions prove how little it has become acclimatized. Almost always we shall find that it is energy integrated over the time or the momentum over the length. Just the same questions are asked about it as used to be asked about the energy. Where does it reside; what becomes of it in a process and so on and so forth? Yet in principle, it has just as good claims as the energy to be considered an ultimate concept, especially when we remember the extraordinary generality of the law of least action which governs every process in classical physics.

Which of these various inter-linked concepts, length, mass, time, energy, action, electric charge, magnetic field, etc. should be taken as the ultimates and which as the derived concepts is a matter of taste. A great number of groups of three can be used as the ultimate indefinables and the others as derived concepts. We have no more real reason to consider length, time and mass as our natural ultimates than we have to consider cartesian co-ordinates the natural ones to use in geometry. It used to be thought that it was preferable to use co-ordinates which one could measure directly. People were under the illusion that length was one of these. But when one comes to exact analysis it is perfectly plain that a whole lot of elaborate physical theories are involved in any accurate measurement of length. The same holds good of time and mass. But if this criterion falls to the ground the apparent obvious pre-eminence of these particular concepts vanishes. Any other suitable group of ulti-

mates would have equal validity provided we could accustom ourselves to thinking in terms thereof. Thus, for instance, mass, charge and action could perfectly well be substituted for mass, length and time. The difficulty would be to get used to thinking in these terms.

Though we are in principle at liberty to choose any co-ordinates or conventions we like in our map of the external world, the aspect we elect to consider frequently renders certain initial conventions much more convenient than others. Just as in geometry no sensible person would describe a spiral in cartesian co-ordinates or a space-lattice in polar co-ordinates, so in physics it seems probable that the most natural description may be preordained by the external world of which we are endeavouring to give a picture. If the ultimates imposed by the nature of the external world happen not to coincide with those to which our mental prepossessions and habits incline us, a conflict will arise. It is this conflict in my view which found expression in what used to be known as the quantum paradoxes.

To understand this, it is necessary to examine the advantages and disadvantages of two very simple alternative modes of description which we can employ in our world map, namely, the continuous and discontinuous. In ordinary macroscopic mechanics there could be no doubt which was the more convenient. The discontinuous form of representation, the description of a system in terms of particles, positions and momenta imposed itself. In the theory of elasticity on the other hand, the continuous equations, fortified by Fourier's proof of the universal validity of harmonic analysis, were obviously the more convenient. In the case of matter the two modes of description were harmonized by admitting that every substance was composed of individual particles or molecules and confining one's applications of the continuous method to regions so large that they contained a considerable number of such molecules. Provided it was realized that the accuracy of the continuous prediction within a given region could only be absolute if it contained an infinite number of molecules, this mode of representation proved perfectly adequate.

For many generations all phenomena associated with light and radiation appeared far more apt to be described by the continuous mode than by the discontinuous. It was only the observation at the end of the last century of the immensely concentrated production of energy in the photoelectric effect which cast doubts upon its universal applicability.

This choice between the continuous and discontinuous modes of description is one of the great problems in considering physical ultimates to-day.

All our early experiences, all our mental prepossessions, almost, one may say, all our natural instincts, lead us to favour the discontinuous form of representation. From our earliest years we have learnt to recognize a lump of sugar, a toy or a stone. Whether it is a projection of our consciousness as individual entities or whatever the cause, this form of picture is so deep-rooted in our nature that it is difficult to overcome it. All space-time description is based upon it; were it not possible in principle to recognize a particle, to fix a point in space, co-ordinate systems would be hard to conceive. Thus the ordinary experimental physicist is extremely loath to adopt universally a continuous form of description of the universe. Though every

physicist would agree that it is possible to represent an atom in the old-fashioned sense, i.e. a concentration of matter, by an infinite number of waves extending to infinity in space and time appropriately arranged to conspire in any desired locality to form a large crest, very few would care to think of the world in such terms. It is this fact of experience, just as valid as any other of our sense data, which inclines us if at all possible to describe the external world in terms of particles.

In the description of ordinary matter, this convention has long been taken for granted. The atomicity of apparently continuous material may have appeared a paradox a 100 years ago, it seems almost self-evident to-day. Indeed it is the breakdown of this mode of description in its simplest form which has shaken metaphysics so severely.

In electricity the atomic mode of description is of much more recent date. As late as 1900, it was held in the German Law Courts that electricity was not a substance, but that to be electrified was a state of material substance, a decision greatly to the advantage of the ingenious individual who was acquitted of theft although he had hooked a conductor on to the overhead wires of the passing tramline company and driven a considerable workshop with the power thus abstracted. It was only since the beginning of this century, when the researches of Sir J. J. Thomson and others gave so many examples of what one would most naturally describe as its atomic characteristics, that the idea that electricity consisted of highly charged particles analogous to the molecules of ordinary matter became popular and indeed universally accepted. As electricity was later than matter to be described in discontinuous terms, so the difficulties of this mode of description were apprehended earlier. Thus diffraction and interference phenomena, so familiar from the study of light, by a somewhat ironic coincidence were observed in the case of electrons, by the son of Sir J. J. Thomson within 30 years of their discovery. Similar phenomena, of course, have since been observed in the case of atoms. We shall revert to these failures of the space-time description of the atomistic picture of the external world later.

Just as the experiments by Sir J. J. Thomson pointed almost inevitably to the necessity of describing electrical phenomena in terms of atoms of electricity, so the host of phenomena associated with the quantum theory have pointed, under the inspiration of Planck, to the necessity of describing action in atomic terms. Many of us have a great difficulty in grasping this. We think of action not as an ultimate indefinable, but as a derived concept made up of components such as momentum and length and energy and time or angular momentum and angle. It seems to us obvious that each of these components can be varied continuously and almost absurd therefore that their product should be discontinuous. But this is largely a matter of habit of mind. Potential and capacity after all can also be varied continuously or for that matter length and breadth and height of a solid body. Yet nobody allows these facts to discourage him from describing electricity and still less ordinary matter in atomistic terms. Some slight alteration in the definition of action may be necessary, possibly mainly, in order to avoid offending our prejudices, just as a certain latitude in placing the zero of energy is frequently convenient. But the desirability of treating

action as atomic, at any rate, when we are determining the course of a process by the Law of Least Action, seems indubitable.

The assumption that action is atomic causes a complete metamorphosis in our outlook. For we wish to represent the world in terms of space and time. Were we content to forgo this, we might of course, describe a hydrogen atom in the ground state simply by saying it is one proton, one electron and one atom of action. But, as has been said, our mental elasticity is insufficient for us to alter so fundamentally our concepts. The ordinary physicist insists, probably rightly, on space and time as his ultimate concepts. Atomic action forces him to reconsider the validity and indeed possibility of doing this.

Let us take the concept of space for instance. Space, like any other concept, is an invention of the human mind to deal with sense data. Sense data are given us by our experiences and are in the widest sense the result of experiment. According to the classical physics, which are, at any rate, approximately accurate, any experiment involves a process governed by the law of least action, i.e. some ascertainable amount of action occurs. In any observation of a length therefore an amount of momentum also is involved.

According to the classical way of looking at it one could plot the action integral for every conceivable mode of transition from the initial state to the final state, these two, of course, being defined by positional and momentum co-ordinates. The mode of transition which would actually be observed, would be the one for which the action integral was a minimum. Geometrically, this could be represented as a curve the lowest point of which would define the process which actually occurs.

Now if action is atomic, it is clear that this action curve will be replaced by a series of steps approximating in their general envelope to the old-fashioned curve. But one sees at once what an immense difference it makes to one's whole metaphysical outlook. For a series of steps of this nature, instead of having a minimum, has a region in which all the points have the same lowest value. Hence the perfectly definite assertion which classical physics was able to make as to the process which would be observed, is replaced by a much more indefinite statement merely giving a region within which the observed phenomena might be expected to occur. This result has effects of fundamental importance, for it implies that the old sharp four-dimensional Minkowski's world lines are degraded to rather indefinite broad and washy regions.

It has an even more far-reaching result. From the classical point of view a system required for its description the exact statement of the positional and momentum co-ordinates of every particle. But this did not rule out the possibility of stating these co-ordinates independently of one another. A concept is meaningless unless it is in principle possible to observe the quantity which it typifies. Even according to classical physics, of course, any observation implied an experiment involving a certain amount of action. Hence, in general, the observation of a length would imply that the momentum was affected and *vice versa*. But according to classical physics, the amount of action could be in principle infinitesimal. Hence either co-ordinate could be observed without affecting the conjugated co-ordinate.

If action is atomic this possibility vanishes. A finite amount of action must be involved and consequently whichever co-ordinate is observed, a finite effect must be produced upon the conjugated co-ordinate. We have said above that it is meaningless to discuss the concept of an unobservable quantity. It is equally unjustifiable, if the observation of the quantity which we are erecting into a concept implies inevitably something about any quantity which can also be regarded as a concept (i.e. the conjugated co-ordinate), to make a statement about the first quantity leaving out of account the second. Our concept of length is formed as a result of our being able to measure length. If every observation of a length implies that the particles whose position is in question must have a momentum, it is not permissible to divorce length from momentum and treat it as an independent concept. If action is atomic no observation of position is even in principle possible without momentum being conveyed to the object whose position is being measured. It follows that every statement about position implies a statement about momentum. To employ the concept of length alone, neglecting the momentum aspect of the observation on which the concept of length is founded, is logically wrong. The error caused thereby may be small, as indeed it is in macroscopic physics, but it is always in principle present and all the major quantum paradoxes have resulted from its neglect.

It should perhaps be emphasized that this point of view deviates fundamentally from that generally held. Ever since Heisenberg enunciated the Principle of Indeterminacy it has been recognized that the observation of one conjugated co-ordinate affects the other. I desire to make it plain that I go a step further than this. I maintain that since the legality of forming or using a concept depends upon the possibility of observing the quantity in question, it is absolutely illogical to endeavour to introduce as an independent concept any quantity, if it is certain that the observation thereof would imply something about another quantity. If that is so you have two linked concepts and any statement about the one implies something about the other. This is not because we have observed it or anything of that sort, it is simply because, since the quantity involved of the first concept could not have been observed without producing some effect on the quantity concerned in the second concept, we have no right to treat either of them as independent quantities and as such endow them with the status of independent concepts.

We know that conjugated co-ordinates are concepts of this type. This being so it is to my mind completely illogical to talk about, say, the distance between two objects or the energy of a system at a given moment. If one says "object $A$ is 1 cm. from object $B$" one must complete the sentence "and has the probable momentum $X$". Similarly, if one spends a long time $t$ in measuring the energy of a system one can say to within a great degree of accuracy that its energy is $E$; but it would be quite meaningless to say the energy at a given moment was $E$. There is no sense in stating the energy at all apart from the time. Energy *per se* is a faulty concept.

The accuracy with which the energy can be stated depends upon the length of the time available. At any given instant of time it may have any value from nought to infinity. The probable error of course diminishes as the time over which it is measured increases. But at best if it were possible to plot it as a function of the time

one would find a fluctuating quantity whose deviations from the mean might have any value.

Exactly the same considerations, of course, apply to any pair of conjugated co-ordinates. Just as a statement about the time implies a statement about the energy, a statement about the angular position implies a statement about the angular momentum. Our minds cannot use the action concept as an ultimate and we are entitled if we wish to use other concepts derived from it; but if it is impossible to represent action by a continuous expression, then no concept derived from it is valid, divorced from its conjugated concept.

The question of course at once arises: what does a statement about one co-ordinate imply about the conjugated co-ordinate? The answer obviously depends upon the type and degree of broadening to which the Minkowski's world lines are subject. At first sight, one might expect an ordinary fading out according to some form of error law. We shall see later on that this over-simplifies the problem. What is clear, however, is that a statement about one conjugated co-ordinate implied by a statement about the other conjugated co-ordinate can never be an exact statement, but can only give a probable value. It may be and, of course, is possible to state the average value, but in any given instance only the probability of observing a given result can be predicted.

The recognition of this logical necessity, namely, that any statement about one co-ordinate implies something about the conjugated co-ordinate invalidates at one stroke many of our most cherished mental prepossessions. Take, for instance, the fact mentioned above that it is no longer possible to talk of the energy of a system at a given instant of time. There is no real reason why this should offend us. We all think we know what the potential of a charged conductor is. But the potential of a conductor containing only one electron is a very indefinite quantity. All our concepts are derived from experiments made with macroscopic objects in which we are dealing with immense numbers of entities. It is scarcely surprising that they should be of their very nature statistical and that difficulties should arise when we try to apply them to individual particles.

This consideration brings us to the idea of distinguishable states. Classical physics would have endeavoured to describe hydrogen atoms in spatio-temporal terms by describing the shape and size of the orbit on which the electron moved and stating its momentum. What we have said shows that such a procedure is not only in practice, but in principle impossible. Any statement about the size or shape of the orbit automatically implies something about the momentum. A very accurate statement involves a very large momentum, often one which is much too large to be compatible with the orbit in question. Such a statement is simply devoid of sense. All we can do is to differentiate between a number of distinguishable states, defined by the condition that we can distinguish one state from the other without changing the system from one to the other. Bohr energy levels, as it turns out, are the mean values of the energy of the system in each of these distinguishable states.

The broad outlines of what we may call the major quantum phenomena all emerge automatically from this point of view. The energy in a system at the ab-

solute zero whose existence seemed so paradoxical 20 years ago becomes an obvious necessity. For we elected to draw our map in terms of particles; as we have seen, the mere fact that we can state that the particle is within the system, i.e. that its position can be stated to a given degree of accuracy, implies as the necessary corollary that it has at least a given amount of momentum and consequently of energy. The rotation of, say, a diatomic molecule equally follows as a matter of course from the mere statement that it *is* a diatomic molecule. For a diatomic molecule is a system not spherically symmetrical. The introduction into our description of the hypothesis that there is such a system is equivalent to introducing something capable of defining an angle and as a logical necessity the statement that we have something capable of defining an angle carries with it the subsidiary statement and therefore having angular momentum. The so-called quantum states are merely the various distinguishable states and the various forms of the quantum theory merely represent accurate methods for inserting into the classical theory the correction which the classical assumption, that a statement about any co-ordinate alone was possible, requires.

There is, of course, a certain logical hiatus in continuing to use classical laws, once one has realized the necessary implications produced by the atomicity of action of any statement about a co-ordinate. For the classical laws are all stated in differential form, i.e. an infinitesimal change in one co-ordinate corresponds to an infinitesimal change in the conjugated co-ordinate. As we have seen, the statement that we know any co-ordinate very accurately implies a large but uncertain value of the conjugated co-ordinate so that an equation implying that we know one co-ordinate with only an infinitesimal error involves an infinite uncertainty in the conjugated co-ordinate. Nothing therefore could be further from logical consistency than to utilize differential equations connecting two conjugated co-ordinates when we know that far from an infinitesimal change in one corresponding to an infinitesimal change in the other, the mere use of an infinitesimal in the one implies an infinite value of the other.

The classical body of knowledge, of course, was derived from experiments with a very large number of particles and represents the average effect, for a large number of individual entities are concerned. This is harmonized with the new point of view by assuming that the average result of a large number of consecutive experiments will approach the average value for a large number of particles with which the experiment is done simultaneously. Everyone will agree that the same result will be obtained by tossing a million pennies simultaneously as will be found if a penny is tossed a million times consecutively. It is this assumption that we have made in what is called the Correspondence Principle and though there is no *a priori* justification for it, nobody is likely to quarrel with it.

This line of approach of course differs decidedly from that of the mathematician. He takes the view that the form of the classical differential equations is everything and must be maintained at any cost. In order to achieve agreement with experiment he is quite prepared to alter the interpretation of the symbols and indeed their very meaning. Thus a perfectly well-defined physical quantity, which one would normally

expect to be expressible as an ordinary number representing the ratio of this quantity to one's conventional unit, is translated (in the sense of the *Midsummer Night's Dream*) into an imaginary differential operator. It would be ungracious to complain of this; it gives the right results. But unless some better reason, some explanation more intelligible to the experimentalist, is provided I shall stick to my interpretation of what the process implies.

To recapitulate, the external world appears to be so constituted that it does not lend itself to description using the ordinary ultimate concepts of physics, namely mass, length and time in the classical way, since this implies that action can vary continuously. If we insist on using these ultimates on account of our mental conservatism, then we must realize that any statement about them implies a statement about the conjugated co-ordinates and due account of this must be taken. When this is done most of what we may call the major quantum phenomena emerge. Unfortunately this is not all.

As mentioned above, it would appear natural that the connection between the probable correction to the conjugated co-ordinate necessitated by a statement about any primary co-ordinate should be given by a probability expression such as one would desire in the usual way, remembering that the elements of action along the axes are additive. This is not the case. If one wishes to obtain agreement with observation one has to substitute for the ordinary probability relation with a real exponent the same expression with an imaginary exponent. Once again, of course, one can display a lack of curiosity and interest in this result and simply say it is so. I am not at all satisfied that this is the utmost we can do. On the contrary it appears that there is an argument derived from quite a different set of experiments which makes the fact intelligible.

As we have stated repeatedly, one of the ultimate concepts most commonly used in physics and which we all believe ourselves to understand, is time. The most significant thing about time or, at any rate, about our apprehension of it, is the marked difference between past and future. Since, according to almost any description of the universe, a physicist is made up of an immense number of particles, it is natural to attribute his very definite apprehension of a direction in the time axis to physiological phenomena based on the second law of thermodynamics. For in the second law, of course, we have the determining quantity, the entropy which increases always as time goes on.

Now Minkowski 30 years ago showed that all the major equations of classical physics could be put into comparatively symmetrical form if one introduced a new variable whenever the time occurred which was simply the time multiplied by the velocity of light and $\sqrt{(-1)}$. If this were done, he showed that one could describe the external world very simply with four completely equivalent interchangeable variables, three of which were identical with the old-fashioned directions of space and the fourth represented, in the new form, the time axis. In other words, he showed that the simplest description of the external world was achieved by utilizing this new variable instead of our time. The essential difference is, of course, that the new variable corresponds to an imaginary time.

It is this fact, in my view, which explains why the relation between the conjugated co-ordinates is not given by an expression with a real exponent, but by an expression with an imaginary exponent.

We have seen above that the external universe requires for its representation something in the nature of an atomic action. It would seem from what has been said that it would be represented better still by substituting a concept analogous to action in which space and time are on an equal footing. In terms of such a concept, of course, the relation between the conjugated co-ordinates would be an ordinary probability relation such as one would naturally expect.

Owing to our mode of apprehending time, the real exponent corresponding to this probability relation would appear as an imaginary quantity. It follows that our mental need to represent events in terms of the ordinary space and time, which we have learnt to know and love in the nursery, forces a sort of wave aspect on to our apprehension of the external world. If we insist on using something akin to spatial co-ordinates, the simplest representation probably would be in terms of Minkowski co-ordinates with an atomic Minkowski action implying an appropriate error-law connection between the conjugated co-ordinates. We have already made a complication by splitting up our action; by substituting our ordinary time we introduce another. If we take account of this, however, the probability expression obviously becomes an oscillatory one and the whole wave aspect of our experience is comprehensible.

In the foregoing, I have endeavoured to give some idea of the conventions and co-ordinates which I prefer in drawing my map of the external universe. As I have said at the outset, the answer to the question which are the best conventions is entirely a matter of taste. Some people may be content with a series of mathematical expressions which they believe it is impossible to translate into physical concepts. Provided, they say, we can establish our one-to-one relation between these little black marks on a sheet of paper and our sense data we ask nothing more. They are, of course, fully entitled to adopt this attitude. For many years students of thermodynamics were content to accept Clausius's entropy without desiring to relate it to anything else. Its explanation in terms the experimental physicist could understand did not alter it, but merely enriched thermodynamics. I believe that an effort to make the quantum theory intelligible to the experimentalist is justifiable for similar reasons. As long as we are compelled to think in terms of space and time and mass and charge and energy and action, so long will the physicist demand why all these mysterious operator equations and matrices and $q$ numbers have to be introduced. He knows that any conventions in drawing his map are permissible. He wants an explanation in terms of the concepts with which he believes he is familiar. As I have said, I believe the answer to be that the external universe is so constituted that it would be best represented by other ultimates than those which we know so well, say, mass charge and some sort of Minkowski action. In such terms perhaps we should be able to describe it, using nothing but whole numbers. But these concepts are not ones with which the experimentalist can work. He wants to use, as heretofore, his ordinary mass, length and time. If action is in any form atomic, it is obvious that

this will impose the need for a correction in the conjugated co-ordinate whenever a co-ordinate is used. Our ordinary human apprehension of time which the principle of relativity has proved long ago to be differentiated from our apprehension of length mainly by the factor $\sqrt{(-1)}$ imposes an oscillatory aspect on our observations. Once we have realized this we can see the reason underlying the more complex calculations of the theoretical physicist. We do not question their methods or their accuracy, all we want to know is what they are about. Nobody challenges the accuracy of a table of logarithms or questions the methods by which they are calculated. But nobody ought to be content when working out what he will owe his bank in 20 years' time, merely to look out the figure opposite the figure representing his overdraft and the figure opposite 1·05 to multiply the latter by 20, add the two together and look out the figure to which the result corresponds. Most of us would ask what the whole process amounts to and why it gives the right result.

The purpose of this lecture will have been achieved if it stimulates physicists to display a similar lively interest when dealing with the mathematicians. Whether such a display of curiosity will render the client popular with the directors of his bank or the physicist popular with the mathematician is of course a totally different question.

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# SECONDARY EMISSION FROM COPPER DUE TO SLOW POSITIVE IONS OF ARGON

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*ABSTRACT.* The dependence of the secondary emission from the cathode on the energies of the impinging positive ions is of importance in the mechanism of the corona, or asymmetric, discharge through gases. In particular, it is important to find whether positive ions are able to liberate electrons from an electrode on to which they drift when their mean energy of agitation is no greater than that of the atoms of the gas through which they move. This paper describes experiments performed with positive ions of argon moving in that gas under weak electric fields in order to measure the secondary emission produced at a copper cathode. No secondary emission was detected when the electric intensity $Z$ was less than 20 V./cm. and $Z/p$ was less than 40, but the secondary emission attained a value of about 1·1 per cent when the electric field was increased above 60 V./cm. corresponding to values of $Z/p$ greater than 100. This result is compared with those previously obtained with hydrogen and helium ions under similar conditions.

## § 1. INTRODUCTION

The emission of electrons from metals under the impact of positive ions plays an important part in the mechanism by which the glow discharge is maintained. The relative importance of this process and that of the ionization of the gas molecules by collision with positive ions varies from gas to gas, but it would appear that the process of secondary emissions practically always occurs to some extent at the cathode of a discharge tube.

Suppose that the average number of electrons emitted from a metal by the impact of one positive ion be called $\gamma$. It is reasonable to consider that $\gamma$ in any given case can only depend on the nature of the metal, the nature of the positive ion, and its kinetic energy at impact. Now in order to derive information about the mechanism of this process it is necessary to find how $\gamma$ varies with the kinetic energy of the positive ions, and also to find how it depends on the ionization potential of the atom from which the ion is formed. It has previously been suggested that the process of electron emission under positive-ion bombardment is one of electrostatic attraction of the electrons from the metal due to the electric field of the ion[1,2], and a theory based on this view has been suggested[3]. If this were the case it would follow that electron-emission could occur when positive ions arrive at the target with the smallest possible kinetic energy. Now it is well known that ions with kinetic energies greater than 100 volts can cause the emission

of electrons from electrodes, and the value of $\gamma$ is known to be a function of the kinetic energy of the ions; but the effects of positive ions having much lower energies have not been studied so fully.

When the positive ions move through a gas, as in the glow discharge, their energy of agitation is, in general, some function of the ratio of the electric force $Z$ to the pressure $p$ of the gas. This function depends on the many processes which may occur when an ion collides with an atom, but the ratio $Z/p$ may be taken as a suitable parameter. It is therefore important to know how $\gamma$ varies with the values of $Z/p$ near the cathode, if the conditions obtaining in discharge tubes are to be reproduced as much as possible. The knowledge of the dependence of $\gamma$ on $Z/p$ when $Z$ is low is important, again, if a consistent theory of the asymmetric discharge is to be formulated.

In the work described in this paper the values of the coefficient $\gamma$ were measured for copper with positive ions of argon moving through the gas under electric forces between 20 and 90 V./cm. when $Z/p < 150$.

## § 2. DESCRIPTION OF THE APPARATUS

The apparatus, illustrated in figure 1, comprised three circular discs $A$, $B$, and $E$ of thin copper 4 cm. in diameter with cylindrical rings round their edges, and two semicircular copper plates $C$ and $D$ 3·5 cm. in diameter and 0·5 mm. thick. The electrodes were supported and insulated from one another on four pyrex rods not shown in the figure. The distance between the discs $A$ and $B$ was 1·7 cm., and

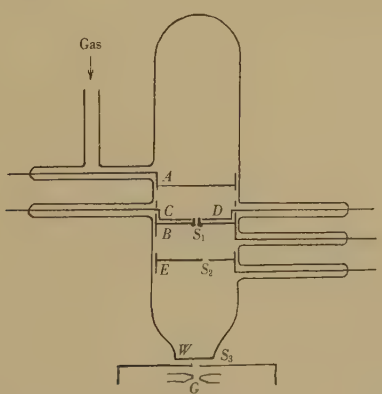


Figure 1. The magnetic field is perpendicular to the plane of the paper.

between $E$ and $B$ 2·25 cm. The electrodes $C$ and $D$ were 0·5 mm. from the disc $B$ and were insulated from it by thin plates of pyrex. The whole was enclosed in a pyrex cylinder so that the surfaces of electrodes were perpendicular to the axis of the cylinder.

There was a slit $S_1$ at the centre of the disc $B$ 1·25 mm. wide and 9 mm. long, while the disc $E$ contained a slit $S_2$ 2 mm. wide and 9 mm. long, situated 0·5 cm. off its centre. The edges of these slits were parallel to the straight sides of the

semicircular plates $C$ and $D$. The slit $S_1$ was fitted with a lip so arranged that the beam of positive ions was not intercepted by the plates $C$ and $D$. Connexions to the electrodes were taken through tungsten-pyrex seals in the side tubes. The end of the tube was fitted with a quartz window $W$ by means of a pyrex-quartz graded seal. This window allowed ultra-violet light from the spark $G$ to pass through the slit $S_2$.

The apparatus was evacuated by means of a mercury-vapour diffusion pump backed by a Hyvac oil pump, and the pressure was measured by means of a McLeod gauge. A liquid-air trap was fitted between the tube and the pumping and measuring apparatus, to prevent mercury vapour from diffusing into the apparatus.

The gas system consisted of a reservoir of commercially pure argon, a pyrex tube containing palladium black, a tube containing charcoal, and two quartz tubes containing metallic calcium. The first quartz tube was fitted with an electric heater so that the calcium could be kept at a red heat in order to absorb impurities from the argon. The second quartz tube was interposed between the last greased tap and the pyrex tube containing the apparatus; and it was sealed to it by means of quartz-pyrex graded seals. The purpose of the calcium in this tube was to prevent the diffusion of any vapours from the tap grease into the apparatus. Repeated heating of this tube was sufficient to maintain the gas quite pure over long periods.

## § 3. EXPERIMENTAL PROCEDURE

The gas used in these experiments was allowed to stand over the palladium black for some time in order that the hydrogen which is present in the commercially pure argon might be absorbed. The charcoal tube served as a pump to circulate the argon over the red-hot calcium, a method due to McCallum and Klatzow[4].

The glass parts of the gas system and the charcoal were thoroughly degased by heating them up to 300° C. for long periods with the pumps evacuating. Both tubes of calcium were kept at a red heat until they gave off no gas at this temperature.

The apparatus containing the electrodes was wound with an electric heater and kept at 360° C. for a week while the tube was kept evacuated in order to take the water vapour out of the glass. After this, the apparatus was further heated to a high temperature by means of a blow pipe, and also by maintaining a discharge between the electrodes and afterwards pumping out the gas. Care had to be taken in this operation as copper sputters very easily.

The light from the spark $G$ passed through the slit $S_2$ and fell on to the underside of the electrode $B$ and liberated photoelectrons, which were accelerated to the plate $E$ by the electric field between $E$ and $B$, and so ionized the gas. The positive ions were attracted to the plate $B$ and some of them were drawn through the slit $S_1$ by the field between the plates $A$ and $B$. The light was limited to a narrow beam by a slit $S_3$ in the screen surrounding the spark gap, so that no light from the spark gap entered the apparatus except through the slit $S_2$. The position of the beam of light on the plate $B$ was so adjusted that no direct light passed through the slit $S_1$,

and it was found that in a certain critical position the light scattered through the slit $S_1$ was negligible. The potential between $E$ and $B$ was so arranged that a positive-ion current of sufficient intensity was received by the plate $A$. This potential was found to be about 10–20 V. less than the sparking potential between $B$ and $E$.

The action of the positive ions after passing through the slit $S_1$ was determined by measuring the negative current received by the electrode $C$ which was at the same potential as the electrodes $D$ and $E$, and comparing it with the positive-ion current received by $A$.

The electrodes $B$, $C$ and $D$ were kept at earth potential and $A$ was maintained at a negative potential $V$, so that the positive ions passing through the slit $S_1$ moved towards the disc $A$ under a force $Z$ equal to $V/1{\cdot}5$, the distance of $A$ from the electrodes $C$ and $D$ being 1·5 cm. The surfaces of the plates $C$ and $D$ and the edge of the slit $S_1$ were all coplanar so that the field $Z$ was sensibly uniform except at the edges of the electrodes. The connexions to the electrodes were all protected from leakage over the glass by external guard rings only, as it was found that owing to the length of the side tube (10 cm.) internal guard rings were unnecessary. *V*

When the force $Z$ was sufficiently large, electrons were set free from the electrode $A$. These electrons were deflected by means of a uniform magnetic field maintained in the whole of the space between the electrodes $A$ to $E$ by two large coils on either side of the pyrex cylinder. The deflected beam of electrons fell on the plate $C$ or on the plate $D$ when the field was reversed. Owing to the very high value of the lateral diffusion of the electrons in argon, it was found to be impossible to deflect the whole of the beam on to $C$. By reversing the magnetic field it was found that at the optimum field-strength 10 per cent of the electrons was received by the other plate $D$. A correction was applied to all the values of the electron currents received by the electrode $C$, and in order to allow for all possible losses of electrons an addition of 15 per cent was made to the observed electron current. The optimum magnetic field was that field which gave the maximum ratio of the electron current received by the plate $C$ to the positive-ion current received by $A$.

The negative charge received by $C$, and the positive charges received by the plate $A$, were measured by means of an electrostatic balance comprising a sensitive electrometer connected to screened air condensers and a potentiometer, and so arranged that the potentials of the electrodes did not alter during the measurements. It was found that for the critical position of the spark $G$ the electrodes $C$ and $D$ received no charge when the potential between $E$ and $B$ was small, say 20 V., and a strong field maintained between $A$ and the electrodes $C$, $D$. Such charges would have been due to diffused light entering the space $AB$. Hence this shows that no charge or ultra-violet light entered the space between $A$ and $B$ unless positive ions were produced by ionization in the space between $B$ and $E$. Moreover, when the positive ions passed through the slit $S_1$, to fall on $A$, no positive charge was intercepted by either of the electrodes $C$, $D$. The positive-ion currents passing through the slit $S_1$ were of the order of $10^{-10}$ A.

## § 4. THE EXPERIMENTAL RESULTS

The measurements were carried out for values of $Z$ from 10 to 90 V./cm corresponding to values of $Z/p$ less than 180, for which the mean energies of the positive ions in argon have been measured and are known to be low[5]. The low values of $Z/p$ were obtained by using low values of the electric force rather than by using high gas pressures with the consequent need of high electric forces. When a large positive charge was received by the electrode $A$ a negative charge could not be detected on $C$ until $Z$ reached a value of about 25 V./cm. and $Z/p$ a value of 50, and further the magnitude of any negative charge collected by $C$ was always proportional to the magnitude of the positive charge received by $A$. Hence the electrons collected by $C$ were produced as a result of the action of the positive ions on $A$. This point was further tested with an apparatus[3] in which the part of the electrode $A$ on which the positive ions fall could be removed by a magnetically operated slider. The positive ions could then pass through the opening and be collected on another electrode. Within the range of $Z/p$ used the negative charge collected by $C$ was always proportional to the positive charge received by $A$.

As the electrons which are set free from the cathode $A$ move through the gas towards the electrode $C$ they produce more electrons by ionization by collision. $n$ Hence the negative charge $n$ received by $C$ in any interval of time is greater than $n_0$ the negative charge $n_0$ liberated at $A$ by the positive ions during that interval. The relation between these charges is given by the well-known Townsend equation

$$n = n_0 \epsilon^{\alpha x} / \{1 - \gamma (\epsilon^{\alpha x} - 1)\} \qquad \ldots\ldots(1),$$

$x$ where $x = d - \delta$, $\delta$ being a certain minimum distance before ionization commences $\alpha$ and $d$ the distance between the parallel electrodes $A$ and $C$, while $\alpha$ is the coefficient of ionization by electrons. We are not here concerned with the exact process of ionization represented by the coefficient $\alpha$; it is sufficient to know that values of $\alpha$ have been determined experimentally for the case in which the above relation represents the growth of photoelectric currents between parallel plates under conditions similar to those discussed here. The presence of the magnetic field modifies to some extent the values of $\alpha$ obtained when the magnetic field is absent owing to the increased distances traversed by the electrons, but this modification is so small that it can be ignored here.

$q$ If $q$ is the total positive charge received by $A$ in the given time interval, then, remembering that

$$n_0/(q - n) = \gamma,$$

we find that

$$\gamma = n/(q\epsilon^{\alpha x} - n) \qquad \ldots\ldots(2).$$

The values of $\gamma$* found from this relation by measuring $n$ and $q$ at various values of $Z/p$ in a number of experiments are shown in figure 2.

* *Footnote added August* 8, 1936. We are very much indebted to Dr S. P. McCallum for providing us with recently checked values of the coefficients $\alpha$ and $\gamma$ in pure argon for various values of $Z/p$, obtained by measuring the growth of currents, and for giving us access to the MSS. of unpublished papers. These values of $\alpha$ have now been used with equation (2) in order to calculate the values of $\gamma$ given in figure 2. If the earlier determinations of $\alpha$ are used $\gamma$ works out at about 15 per cent lower. See, for instance, Ayres, *Phil. Mag.* **45**, 353 (1933).

Now in deriving the above expression for $\gamma$ it is assumed that all the electrons liberated from the cathode $A$ finally arrive at $C$. However, in general, this does not occur, because the electrons in the space $AB$ diffuse in all directions, and further the process of diffusion is very marked in argon. Those electrons which diffuse back to the cathode are recaptured there, and so are removed from the current. Hence it is necessary to investigate this process more fully before the values of $\gamma$ given in the curve can be considered to represent the actual number of electrons liberated at the cathode per positive ion. In the above expressions it merely represents the number of electrons which actually move away from the cathode and contribute to the current.

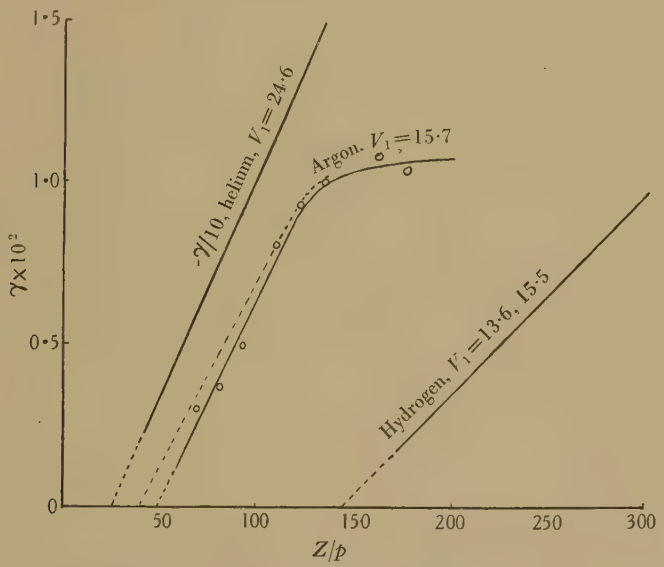


Figure 2. Results obtained with copper cathode.

Now the scattering of photoelectrons back to the cathode from which they were emitted has been investigated theoretically by Young and Bradbury[6], who expressed the ratio of the actual photoelectric current $i$ to the saturation value $i_0$ of the current in terms of the ratio $Z/p$. They found that $i, i_0$

$$i/i_0 = f(a), \qquad a$$

where

$$a^2 = LZ/E_0 p.$$

Values of the ratio $i/i_0$ corresponding to various values of the expression $f(a)$ are given in their paper.

$L$ is the mean free path of the electrons in the gas at a pressure of 1 mm. and $E_0$ is the energy, in electron-volts, with which they are emitted from the cathode. In determining this function it was assumed that the return of an electron to the cathode was determined by the first collision with a gas atom after emission. Let us apply this relation between $i/i_0$ and $a$ to the results given in figure 2 in order to determine how far the currents were from saturation. We must first estimate the $L$ $E_0$

value of $E_0$. From the experiments of Oliphant[3,7] it is known that although there are some electrons with energies $V_1 - \phi$, where $V_1$ is the potential energy of the ion and $\phi$ the work function of the cathode, most of the electrons have energies of only one or two volts. Thus assume $E_0$ to be 1·5 volts and let $L$ be taken as 1 mm approximately[8]; then when $Z/p = 150$, $a^2 = 7{\cdot}5$ and $i/i_0 = 0{\cdot}85$, and for the lower limit $Z/p = 50$, $a^2 = 2{\cdot}5$ and $i/i_0 = 0{\cdot}75$. From this it is seen that correction for the backward scattering of the electrons results approximately in the displacement of the curve to the left into the position shown by the dotted line.

## § 5. THE DISCUSSION OF THE RESULTS

The secondary emission produced by argon positive ions has been measured previously by Penning[9], who obtained values of $\gamma$ for ions of energies of the order of 80 volts striking a degased cathode of nickel *in vacuo* ($\phi = 5$ V.). By extrapolating the {$\gamma$, energy} curve to zero energy it was found that a probable value of $\gamma$ for ions of very low energy was 0·005. It is seen that this value is in agreement, as far as order of magnitude is concerned, with the values given in the curve, figure 2. Now on the assumption that even under intense electric fields the ionizing action of positive ions is confined to the liberation of electrons at the cathode, it is possible to calculate values of $\gamma$ on the Townsend theory of sparking by equating the denominator in equation (1) to zero. However, it is not easy to obtain accurate results in this way owing to the large variations in sparking potential with distance and the consequent uncertainty in the exact value of $Z/p$. Further, to find $\gamma$ at low values of $Z/p$ it is necessary to use high gas pressures and therefore intense electric fields. McCallum and Klatzow[10] found values of $\gamma$ from 0·004 to 0·005 for argon ions striking a nickel cathode when $Z/p$ lay between about 100 to 250; and a value of 0·018 when $Z/p$ was 40. Again, from the sparking-potential curves of argon given by Penning and Addinck[11] values for $\gamma$ of 0·005 when $Z/p = 70$ and 0·018 when $Z/p = 20$ are found for an iron cathode; here $\phi = 4{\cdot}7$ V., $Z = 500$ V./cm. and $p = 25$ mm. A more accurate method of estimating $\gamma$ is to measure the rate of growth of photoelectric currents in the gas. In this way McCallum and Klatzow[10] found $\gamma$ to be about 0·004–0·005 with values of $Z/p$ between 80 and 175, again with a nickel cathode.

The secondary emission from copper, measured as a function of $Z/p$, has previously been determined with positive ions of hydrogen[12] and also with positive ions of helium[13], and these results are given for comparison in figure 2. From the shape of the two curves it would appear that extrapolation to low values of $Z/p$ is reasonable, and from this it follows that $\gamma$ becomes zero in hydrogen when $Z/p < 100$, and in helium when $Z/p = 15$, but correction for the backward diffusion of the electrons would reduce this value. Further, if extrapolation is justifiable here it follows from figure 2 that in argon $\gamma$ decreases to zero when $Z/p$ is less than about 40. In fact, in these experiments with argon any charge received by the collecting-electrode $C$ during an interval of about 10 sec. could not be measured on the electrometer, when $Z = 20$ V./cm. or $Z/p < 45$. Again in the experiments

with hydrogen ions no secondary emission was detected when $Z/p < 80$. Hence these curves lead to the conclusion that electrons are not emitted from an electrode in a gas by the impact of positive ions when these have energies not very different from that of the gas atoms.

Now there is evidence[7,9] to indicate that some secondary emission does occur with ions of low energy striking a degased electrode *in vacuo*; but these conditions are not those obtaining in a discharge through a gas. Further, it is well established that the secondary emission is profoundly affected by the state of the surface of the target. However, the present experiments with argon and the earlier ones with hydrogen and helium lend support to the idea that when the cathode is immersed in a gas, as is the case in a gas-discharge tube, and the electric intensity near the cathode is not very great, the escape of secondary electrons does not occur, whereas it could occur without hindrance if the cathode were degased and *in vacuo*.

It is now interesting to consider the nature of the ions employed in the previous experiments with hydrogen. From the experiments of Smyth[14] and of Hogness and Lunn[15] it would appear that the primary product of ionization in the region *EB* of the apparatus is the ionized molecule $H_2^+$, but that the atomic ion $H^+$ is readily produced by the dissociation of the molecular ion in subsequent collisions and that $H_3^+$ ions may be formed. Further, the work of Kallman and Bredig[16] shows that this atomic ion tends to persist, since they found that the ratio $H^+/H_2^+$ increased when the pressure of the gas was increased from $0{\cdot}1 \cdot 10^{-2}$ to $1{\cdot}0 \cdot 10^{-2}$ mm. Moreover, it would be difficult for a massive $H_3^+$ ion to attain high energies owing to collisional losses, and the process of electron-exchange would tend to limit the energy of the $H_2^+$ ions as they move through the gas, but the effect of this process in limiting the energies of the atomic ions would be much less[17,18,19]. Again, the work of Bleakney[20] and Lozier[21] has indicated the presence of high-speed atomic ions in hydrogen produced by electrons with energies of about 30 eV., and such electrons should be plentiful when $Z/p$ is about 300. Hence it is reasonable to suppose that the positive ions which were received by the copper cathode in the experiments with hydrogen contained a large proportion of atomic ions when they had attained high energies under the fields used in those experiments, in which $150 < Z/p < 400$. This view is supported by the fact that copious ionization by the positive ions by collision with the gas molecules was recorded. However, these considerations make it all the more remarkable that the secondary emission due to the hydrogen ion was so small when compared with that due to the argon ions and the very efficient helium ion, because the kinetic energies of the hydrogen ions were certainly very much greater than those of the argon ions at the same values of $Z/p$. It would seem probable that this difference in efficiency of the various ions is due partly to their different potential energies. Thus on this view the helium ion, for which $V_1 = 24{\cdot}6$ V., would then be more efficient than the hydrogen atomic ion, for which $V_1 = 13{\cdot}5$.

§ 6. ACKNOWLEDGEMENTS

We wish to thank Dr S. P. McCallum for very helpful conversations and for giving us access to some unpublished results, and to Prof. E. J. Evans for extending the facilities of his laboratory.

NOTE ADDED IN PROOF

In a recent paper, *Physica*, **3** (6), 515 (1936), Kruithoff and Penning have estimated $\gamma$ for argon ions striking copper by measuring the growth of photoelectric currents. Values of $\gamma$ from $8{\cdot}8.10^{-3}$ to $3{\cdot}76.10^{-2}$ were found, but there was no consistent variation with $Z/p$. However, in one experiment an average value of $1{\cdot}2.10^{-2}$ was obtained, which agrees well with the maximum value $1{\cdot}1.10^{-2}$ found in the present direct experiments.

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# WIRELESS WAVES REFLECTED FROM THE IONOSPHERE AT OBLIQUE INCIDENCE

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*ABSTRACT.* The waves emitted by a pulse transmitter at Cambridge were simultaneously received at a point distant 1 km. and at Edinburgh distant 464 km. in a direction 15° west of magnetic north. The variation of equivalent path with emitted frequency was studied for waves of such a frequency that they nearly penetrated the *F* region. The results agreed qualitatively with a simplified theory, and under certain conditions showed the possibility of two transmission paths to the distant point. A quantitative discrepancy between the simple theory and experiment was noticed and its possible explanations are discussed. It is shown that by comparing the frequencies which just penetrate the *E* region at normal and oblique incidence it might be possible to decide whether the Lorentz polarization term should be included in calculating the force on an electron in the ionosphere. Experiments made for this purpose provided only a statistical result on account of the variable nature of the *E* region. Measurement of the sunrise increase of absorption for waves received at the two points indicated that the day-time absorption takes place in a region below the main deviating portion of the *E* region, for echoes both from the *E* and from the *F* region.

## § 1. INTRODUCTION

Most of the experiments in which wireless waves have been observed after reflexion from the ionosphere have been made with the sender and receiver close together, so that the waves were incident on the ionosphere at small angles of incidence. There have, however, been a few experiments in which reception has taken place simultaneously at nearby and distant receiving stations. Appleton and his collaborators[1, 2] and Martyn, Cherry and Green[3] have described experiments of this type in which the Appleton frequency-change technique was used. In these experiments the mean wave-frequency was kept constant and observations were made of the way in which the equivalent reflection height $P'$ changed with the time $t$; this procedure may be referred to as the $\{P', t\}$ technique. The present paper is an account of similar experiments in which the Breit and Tuve pulse method was used, and in which the wave-frequency $f$ could be varied continuously over a wide range; this procedure may be referred to as the $\{P', f\}$ technique.* Two receivers were employed, distant 1 and 464 km. from the sender. The chief points investigated were (*a*) the behaviour of waves of such frequency that they were just about to penetrate the *F* region (§ 3), (*b*) the behaviour of waves just about to penetrate

$P'$ $t$ $f$

* See note at end of paper.

the $E$ region (§ 4), and (*c*) the way in which the absorption depends on angle of incidence (§ 5).

At first sight it might be thought that by comparing the behaviour of waves at normal and oblique incidence it would be possible to investigate the form of the wave-trajectory and the distribution of ionization between the $E$ and $F$ regions. Martyn[4] has, however, shown that if the earth's magnetic field is neglected it is possible to deduce the behaviour of a wave at oblique incidence from that of a wave of a different ("corresponding") frequency at normal incidence, and that this deduction is independent of any particular distribution of ionization. If, therefore, the equivalent height of the wave is known as a function of frequency for normal incidence, then it can be deduced for oblique incidence. Our experimental results concerning the penetration of the $F$ region agree qualitatively with the deductions from Martyn's theorem, but there is a quantitative discrepancy which is discussed below in some detail.

In § 4 it is shown that by comparing the $E$-region penetration frequencies for normal and for oblique incidence it should be possible to decide whether or not the Lorentz polarization term[5] is to be included in calculating the force on an electron in the ionosphere. Experiments made with this object are described, but it is pointed out that, owing to the inconstant nature of the $E$ region, the results are only statistical in nature.

Appleton and Ratcliffe[1] have pointed out that by comparing the absorption of waves for different angles of incidence it is possible to decide whether the absorption occurs in a relatively non-deviating region situated below the level of the main deviation, and they made some experiments, using continuous wave emissions, to investigate this point. There are several advantages in using the pulse technique for this kind of measurement since it enables observations to be made on individual echoes instead of only on the resultant down-coming wave. Experiments of this kind are described in § 5.

## § 2. EXPERIMENTAL ARRANGEMENTS

A pulse-transmitter similar to that described by Ratcliffe and White[6] was situated at Cambridge, and was provided with a series of coils and aerials so that it would cover the frequency range from 6·0 to 0·67 Mc./sec. The aerials were horizontal wires approximating to half-wave aerials for the mean wave-length of each coil. The emitted waves were received simultaneously at a hut in Cambridge distant 1 km., and at Edinburgh distant 464 km. from Cambridge in a direction 15° west of magnetic north.

The tuning-condenser at the transmitter could be varied by means of a distant control from the Cambridge receiving-hut, so that the emitted frequency could be changed continuously over the whole range of one coil. The coils and aerials could also be interchanged by means of relays worked from the receiving-hut. The signal at the Cambridge receiver was made to give a stationary pattern of the usual kind on the screen of a cathode-ray oscillograph, the time base being driven by the same a.c. mains as those which drove the transmitter.

The receiver at Edinburgh was situated in the Physics Laboratory of the University, and at this distance the received signal consisted only of the pulses reflected from the ionosphere, the ground wave being too weak to be observed. It was not possible to drive the time base from the a.c. mains as at Cambridge, because the mains at the two places were not exactly synchronized. It was therefore necessary to use a self-synchronizing time base which was driven by the received signal. This apparatus was developed by one of us (F. T. F.) in collaboration with Mr S. H. Falloon and has been described elsewhere[15].

## § 3. PENETRATION OF THE *F* REGION

To investigate the penetration of the *F* region at oblique incidence, $\{P', f\}$ curves, showing equivalent path as a function of frequency, were plotted at the two receiving stations simultaneously. At Cambridge equivalent paths were measured directly, but at Edinburgh, where there was no ground ray, the differences between

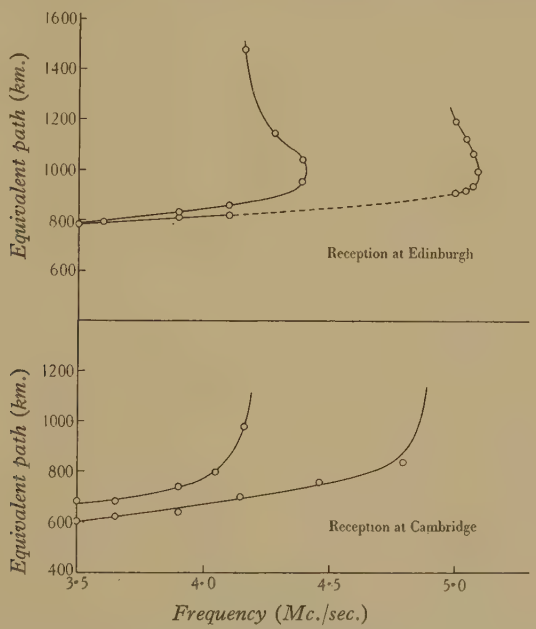


Figure 1. The left-hand curves refer to the ordinary wave, and the right-hand to the extraordinary.

the equivalent paths of the various echoes were measured. As a first approximation it was assumed that the doubly reflected extraordinary wave at Edinburgh was reflected from the same equivalent height as that measured at Cambridge,* and from this the other equivalent heights were calculated. The results of a typical experiment are shown in figure 1. Over the dotted portion of the curve the extraordinary echo alone was present so that it was impossible to estimate the magnitude of the equivalent path, but for those frequencies for which the echo was split the

* The doubly reflected wave has angle of incidence approximating most nearly to the vertical, and the extraordinary wave suffers least group-retardation.

magnitude of the splitting was accurately measured. The dotted portion of the curve has been drawn as a copy of the curve for the ordinary wave.

We turn now to a theoretical discussion of the experimental curve of figure 1. If a $\{P', f\}$ curve is observed at normal incidence, it is possible to deduce the $\{P', f\}$ curve which should be observed at a distant point by making use of a theorem due to Martyn[4], which states that if $P'(f, \theta)$ represents the equivalent path
$\theta$ observed for a frequency $f$ and an angle of incidence $\theta$, and if

$$1-\mu^2=AN/f^2 \qquad \text{.....(1)},$$

$\mu, A, N$ where $\mu$ is the refractive index, $A$ a constant, and $N$ the electron-density,

then $$P'(f\cos\theta, 0)=\cos\theta . P'(f, \theta) \qquad \text{.....(2)}.$$

This relation holds whatever the vertical distribution of ionization. If we relate
$x$ the horizontal distance of transmission $x$ to the other quantities by the expression

$$x=P'(f, \theta)\sin\theta \qquad \text{.....(3)},$$

then from the observed $\{P', f\}$ curve at normal incidence we may calculate $x$ as a function of $\theta$ for a series of different frequencies, by eliminating $P'(f, \theta)$ from (2) and (3). Figure 2 shows this relation for the ordinary wave, calculated from the normal incidence curve of figure 1. From figure 2 it is next possible to construct curves showing angle of incidence as a function of frequency for a fixed distance of transmission, and then, by using the expression (3), curves of equivalent path as a function of frequency may be plotted. This is done in figure 3 for $x=464$ km., to correspond to the case of reception at Edinburgh. From the curves of figures 2 and 3 it is seen that for certain wave-frequencies it is possible to have two rays joining two points on the earth's surface, one with a steep angle of incidence and a flat-topped trajectory, and the other with a less steep incidence and approaching more nearly to an inverted V-shaped trajectory. This possibility has been pointed out by other investigators, and has usually been discussed by considering a $(\sin)^2$ distribution of ionization-density[7,8]. In virtue of the form of equation (1) our calculation neglects the effect of the earth's magnetic field, and does not include the Lorentz term.

The experimental curve for the ordinary wave is plotted with the theoretical curve in figure 3. It is clear that the form of the two curves is the same but that quantitative agreement can only be obtained by displacing the calculated curve towards the smaller frequencies by 0·23 Mc./sec. Four curves of the type shown in figure 3 were obtained on two separate nights, and they all show the same frequency-displacement. In order to plot curves of this type it is essential that the $F$-region echo should be sharply defined, and on several nights the experiments were useless because this was not the case.

The quantitative discrepancy may arise from the neglect of one or more of the following factors: (*a*) presence of the earth's magnetic field, (*b*) the possible necessity for including the Lorentz term, (*c*) variation of ionization-density with latitude, and (*d*) curvature of the earth.

It is well known that at normal incidence the critical electron-density required to reflect the ordinary wave is independent of the earth's magnetic field, but at

oblique incidence this is not necessarily the case. The detailed application of Appleton's magneto-ionic theory to the case of oblique incidence on the ionosphere is complicated owing to the varying angle between the direction of propagation and

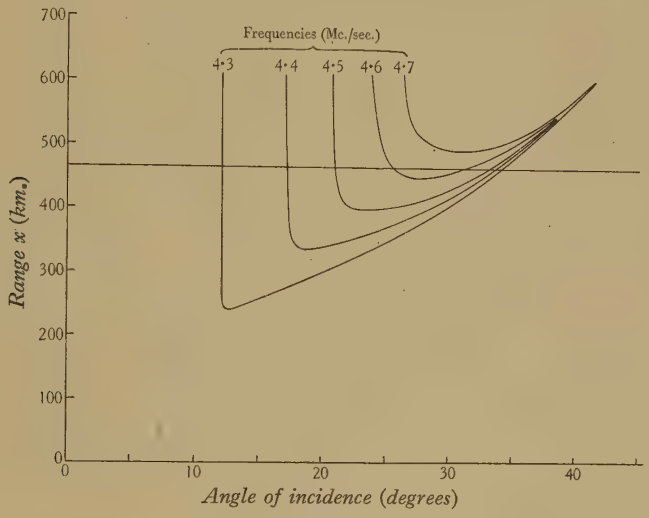


Figure 2.

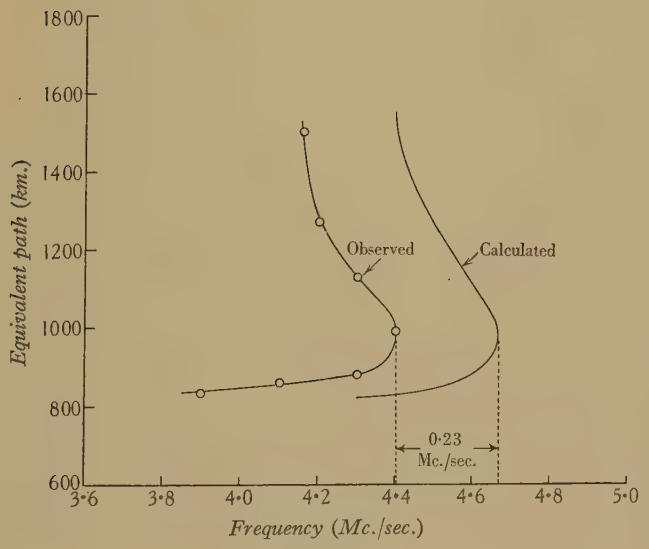


Figure 3.

the direction of the magnetic field, and a full solution has only recently been attempted by Booker[9], who is now trying to adapt his analysis to our special case. We cannot tell yet, therefore, whether the discrepancy is to be explained in this way.

That the inclusion of the Lorentz term also produces a correction which is dependent on the angle of incidence $\theta$ is easily seen by considering reflection from

a slab of uniform ionization-density not subject to a magnetic field. The ratio of the penetration frequency for oblique incidence to that for normal incidence is then

$$\sqrt{\frac{\sec^2\theta - l}{1 - l}},$$

*l* where $l$ has the value 0 or $\frac{1}{3}$ according as the Lorentz term is excluded or included. The effect of including the Lorentz term for reflection from the *F* region, with its gradual variation of ionization-density, is not easy to deduce, mainly because when this term is included the group-velocity is no longer simply related to the phase-velocity. We cannot at present decide whether the effect of including this term would explain our results.

An explanation of the quantitative discrepancy on the basis of a latitude variation of ionization-density would require that the ionization-density should decrease as we go north on a summer night. Reference to the theoretical ionization curves of Millington[10] shows that we should expect the reverse to be the case, but in view of the fact that at a given point the ionization-density has recently been shown to increase during the night[14], it appears possible that it should actually be less in the north where night conditions have not been in existence for so long.

We are satisfied that the curvature of the earth is not responsible for the discrepancy revealed by figure 3.

Further experimental and theoretical investigations are required before the quantitative difficulty can be resolved.

## § 4. PENETRATION OF THE *E* REGION

It has been pointed out in the preceding section that, for a sharply defined slab of uniform ionization, the calculated ratio between the penetration frequencies for normal and for oblique incidence depends on whether or not the Lorentz term is included. Since curves of equivalent height against frequency at normal incidence show that the lower edge of the *E* region is fairly sharply defined, we may, to a first approximation, consider this region to be a sharply defined slab of uniform ionization-density and, by comparing the penetration frequencies of this region for normal and for oblique incidence, it should be possible to decide whether or not the Lorentz term is to be included. The effect of the earth's magnetic field may be eliminated to a first approximation by observing on the ordinary wave only, since for this, assuming the quasi-transverse approximation to the magneto-ionic theory in the reflecting region, the ionic density required for reflection is independent of an external field.

A series of experiments was made to determine the penetration frequencies of the *E* region for reception at Cambridge and at Edinburgh. Difficulty was experienced in estimating the critical frequency as observed at the distant station owing to the fact that partial reflection often occurred for frequencies higher than the true penetration frequency. For normal incidence this difficulty was not serious, since the appearance of an *F* echo could be taken to represent the penetration of the *E* region, even if it was accompanied by an echo partially reflected from the

$E$ region. At oblique incidence, however, this criterion could not be used, since $E$ and $F$ echoes were present together over a wide frequency-range. It is explained in the next section that this is because the $F$-region echo penetrates the $E$ region on account of its steeper angle of incidence. We took that frequency at which the amplitude of the $E$ echo fell considerably below that of the $F$ echo as giving an upper limit for the penetration frequency.

It was found on some nights that the conditions in the $E$ region were not steady, and observations could not be repeated, while on other nights there was so much partial reflection that no precise estimate of the penetration frequency could be made. Out of fourteen nights' experimenting only five were found to yield reliable results. The results obtained on these nights give rise to the values for $l$ shown in table 1.

Table 1. Experimental values of $l$

| Date May 1935 | Values deduced for $l$ |
|---|---|
| 8 | 0·16, 0, −0·3, −0·3 |
| 13 | 0·06, 0·29, −0·3 |
| 14 | 0·03, −0·7 |
| 20 | 0·5, −0·2, 0·07, 0·26 |
| 21 | −0·7, 0·05 |

The mean of these values is 0·05, but in view of the large spread in the readings themselves it is doubtful whether this set of results can decide between the possible values 0 and $\frac{1}{3}$. In considering these results it is further to be remembered that we have assumed that the ionization-density does not vary with latitude.

The penetration frequencies observed at the two points often showed rapid changes which were quite unrelated to each other, indicating that the ionization-densities above Cambridge and above the mid-point between Cambridge and Edinburgh were varying in an unrelated manner. This type of behaviour suggests that the $E$ region is not uniformly ionized in a horizontal direction. If we picture the $E$ region as composed of clouds of more and less dense ionization we can explain these results as well as the results of Ratcliffe and Pawsey [11] on the lateral deviation of reflected waves, and we have a simple mechanism for the partial reflection and partial transmission which is so frequently observed at vertical incidence, without the necessity of assuming an extremely sharp boundary for the layer.

The fact that the electron-density in the $E$ region could sometimes be greater near the mid-point than at the ends of the transmission path was apparent from the frequent occurrence of echoes with an equivalent path corresponding to the type of trajectory shown in figure 4. This type of echo occurred much more frequently for the oblique than for the normal transmission.

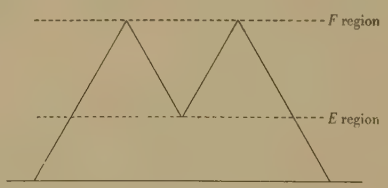

Figure 4.

We conclude therefore that without experimental knowledge of the variations in ionization-density over the mid-point of the trajectory, and in the absence of a

very large number of observations, this type of experiment cannot give a reliable result concerning the Lorentz term.

## § 5. ABSORPTION

Information about the absorption of waves may be obtained by comparing the amplitudes of echoes returned at oblique and at normal incidence. Appleton and Ratcliffe[1] have discussed the way in which the absorption of waves varies with angle of incidence in the two cases (i) where the absorption occurs near the top of the trajectory and (ii) where the absorption occurs in a relatively non-deviating region below the main deviating region. They point out that for case (i) the absorption decreases as the angle of incidence increases,* whereas for case (ii) the absorption increases as the angle of incidence increases.

In considering case (i) they discussed two special types of ionization-gradient and their conclusions have since been verified for other ionization-gradients by Martyn[4] and by Norton, Kirby and Lester[12]. In attempting to decide whether case (i) or case (ii) represents the facts the more correctly, the above-mentioned workers have attempted to measure the amplitude of the down-coming wave for different angles of incidence, but in interpreting the experimental results it has been necessary to make assumptions about the vertical polar diagram of the sending aerial.

In the present series of experiments observations of the amplitudes of the waves reflected from the *E* region were made at normal and at oblique incidence during the sunrise period. The height of the *E* region remained very constant during the period of observation, so that the angle of incidence was constant. By confining attention to the *increase* of absorption at sunrise, results were obtained which were independent of the angular distribution of radiation at the transmitter. The amplitudes of the echoes were measured by means of the length of the traces which they formed on the oscillograph screen, the sensitivity of the apparatus being altered from time to time by means of a calibrated gain-control. This rough measurement was sufficient for the purpose, indeed it would have been difficult to devise a more accurate method on account of the pronounced fading which occurred with all echoes.

Measurements of this kind were made on four separate occasions, viz. 4, 6, 7 and 14 May 1935, the wave-frequency being 1·67 Mc./sec. (wave-length 180 m.). It was found that the effect of absorption was first evident at the distant station about half an hour before ground sunrise, and that one hour after sunrise the amplitude had fallen to about 0·06 of the night-time value. For normal incidence the effects of absorption were not noticed until about one hour after sunrise, and then the amplitude had fallen to about half its night-time value. Three and a half hours after sunrise the amplitude measured at normal incidence was reduced to

* There is a misprint in the middle of page 155 in their paper where it is stated that "the *reflection* coefficient is less for the more distant stations"; this should obviously read "the *absorption* coefficient is less for the more distant stations" in agreement with the equations at the top of the previous page.

0·25 of the night value, whereas the signal was quite unobservable at oblique incidence.

The fact that waves arriving at oblique incidence are strongly absorbed near sunrise, whereas those arriving at normal incidence are absorbed much less, is apparent when one is watching the echoes from a nearby pulse transmitter, for it is very noticeable that the background mush disappears during the half-hour after sunrise, whereas at these frequencies the echo signal does not alter appreciably.

From these results it is quite clear that the day-time absorption at oblique incidence is much greater than that at normal incidence, so that the second assumption of Appleton and Ratcliffe appears to be true, and the absorption occurs in a relatively non-deviating region below the main deviating region. It is interesting to see whether this assumption leads to the observed numerical relation between the absorption at vertical incidence and that at oblique incidence. To calculate the absorption at oblique incidence we consider the up-going and the down-coming paths separately, and we assume that the direction of transmission through the absorbing region is that corresponding to a sharp reflection at a height of 100 km. The value of the absorption coefficient in a region where the refractive index is approximately unity has been given in a previous paper[13], and has been shown to be inversely proportional to either

$$(a)\quad (f+f_H\cos\theta')^2$$

or

$$(b)\quad (f+f_H\cos\theta'),$$

according as one or other of two assumptions is made. Here $f_H$ is the magneto-ionic gyro-frequency and $\theta'$ is the angle between the ray and the earth's magnetic field. $f_H$

We here restrict our calculations to the ordinary wave, since for the frequencies we are considering the amplitude of the extraordinary wave is negligible in comparison. In the particular case considered here it happens that the up-going wave travels exactly at right angles to the earth's magnetic field, so that the quasi-transverse approximation to the magneto-ionic equations should be used. Although the expressions given above are deduced for the quasi-longitudinal case they reduce to the quasi-transverse case when $\theta'$ is put equal to $\pi/2$ so that we may use them as they stand. For a slab of absorbing region the ratio of the integrated absorption coefficients for the vertical and the oblique paths is given by

$$\sec i_0 \left\{\frac{1}{(f+f_H\cos\theta'_1)^2}+\frac{1}{(f+f_H\cos\theta'_2)^2}\right\} \div \left\{\frac{2}{(f+f_H\cos\theta'_0)^2}\right\}$$

in case ($a$) and by a corresponding expression in case ($b$), where $i_0$ is the angle of incidence, and $\theta'_0$, $\theta'_1$, $\theta'_2$ are the angles which the ray makes with the earth's magnetic field in the case of the vertical ray, the up-going oblique ray, and the down-coming oblique ray respectively. $i_0$ $\theta'_0, \theta'_1, \theta$

Calculating in this manner we find the following results for the ratio of the integrated absorption coefficients:

5·45 for assumption ($a$),
3·62 for assumption ($b$).

The observations already mentioned indicate that one hour after sunrise the rat of the integrated absorption coefficients was

$$\frac{\log 0{\cdot}06}{\log 0{\cdot}5}, \text{ which} = 4{\cdot}0.$$

To the order of accuracy of the experiment this may be considered as being i agreement with either of the assumptions.

These results indicate that the absorption which becomes apparent after sunris occurs in a relatively non-deviating region below the $E$ region, and this conclusio is in agreement with the deductions made by Appleton and Ratcliffe[1], wh observed near sunset, and by Farmer and Ratcliffe[13], who observed near th middle of the day. They do not agree with those of Martyn[4] whose observation lead to the conclusion that the absorption is in the topmost portion of the wave trajectory, but it must be remembered that his conclusions were based on night time observations.

When observing at oblique incidence it is possible to receive echoes simul taneously from the $E$ region and from the $F$ region, the wave from the $F$ regio having penetrated the $E$ region because of its steeper angle of incidence. By using these two waves it is possible to investigate simultaneously the absorption suffered by each separately; this can never be done at vertical incidence, since the two reflections do not occur together.* Observations at Edinburgh, made with a wave-length of 180 m. on the $E$ and $F$ echoes, which were visible simultaneously during the sunrise period, indicated that the amplitudes decreased together, the amplitude of the $F$ echo decreasing slightly more than that of the $E$ echo. This seems to indicate that the absorption takes place in the same region for both echoes, and that there is no pronounced absorption of the $E$ echo near the top point of its wave-trajectory. The slight extra absorption of the $F$ echo may be due to its having traversed the upper portion of the $E$ region.

## § 6. ACKNOWLEDGEMENTS

We are much indebted to Prof. Barkla for providing us with very suitable accommodation in his laboratory at Edinburgh, and for making us so comfortable during our stay there. We also wish to acknowledge the help which we have received from the Department of Scientific and Industrial Research, who have made a grant to one of us (F. T. F.) and who have lent us some of the apparatus. We are further indebted to Mr H. G. Booker for many helpful discussions on theoretical points.

## NOTE ADDED IN PROOF

After this paper had been communicated an account of similar work was published in Germany, *Hochfrequenztech. u. Elektroakust.* **48**, 1 (1936), by Crone, Kruger, Goubou and Zenneck. They used the Breit and Tuve pulse technique and the $\{P', t\}$ method of observing, and compared reception at normal and at oblique incidence. The main phenomena recorded in their paper agree with those which we found by means of the $\{P', f\}$ technique.

* Except under conditions of partial reflection, which are not suitable for the present purpose.

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# THE ELECTRICAL CONDUCTIVITY OF BARIUM CHLORIDE AND ITS VARIATION WITH TEMPERATURE

By V. J. JEHU, B.Sc., Queen Mary College, University of London

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*ABSTRACT.* The electrolytic conductances of aqueous solutions of barium chloride have been measured over a wide range of concentration and temperature. The equivalent conductivities at infinite dilution have been found by the method of Shedlovsky, and a formula connecting conductivity and temperature is given.

## § 1. INTRODUCTION

Beyond the recognition of the fact that electrolytic conductivity varies extremely rapidly with temperature, the problem of the temperature variation has received very little attention, in marked contrast with the large amount of work that has been done on the variation with concentration.

Kohlrausch[1] studied the variation of a number of strong electrolytes in water over the restricted range of temperature 0–30° C., whilst, on the other hand, Noyes[2] has carried out measurements over a much wider range extending to 306° C., but in no great detail.

Recently Welton[3] investigated the electrical conductivity of aqueous solutions of potassium and sodium chlorides over a temperature range 18–85° C. Following this path, Clews[4] studied aqueous solutions of potassium nitrate, sodium sulphate and potassium sulphate over the same temperature range. The present paper, forming an addition to this series, deals with a bi-univalent salt, namely barium chloride. This salt was selected because of its convenient chemical properties; the crystals can be dehydrated by heat without any formation of hydrochloric acid, and the salt is not appreciably hydrolysed even in hot aqueous solution.

## § 2. EXPERIMENTAL METHOD

The Wheatstone network employed was that described by Clews[4]; it consisted of a ratio box, a six-dial decade resistance box, and the conductivity cell. All the resistances used were of low inductance, and a very sharp balance point was obtained by the inclusion of a variable condenser in parallel with the measuring resistance. By means of a mercury rocking key the positions of the cell and the measuring resistance could be reversed with respect to the ratio arms.

A one-valve oscillator of the tuned-anode type was again used as source of current, and this possessed a frequency of 1152 c./sec., within the range of maximum

sensitiveness of the ear. As before, a telephone receiver connected to the bridge through a two-stage amplifier was used as detector.

The stronger solutions were made up by weight, but below 0·01 *N* the solutions were prepared by dilution. This was done by weighing out a certain volume of 0·01-*N* solution, syphoning conductivity water from a Hartley still into it, and re-weighing. The densities of the 0·01-*N* solution and of the water at 25° C. being known, the respective volumes could be calculated accurately and hence the concentration of the new solution could be determined. By this means contamination of the solution due to long exposure to the air in the laboratory was avoided.

Washburn cells made in pyrex glass were used, the cell constants being determined by means of the 0·1-*D* and the 0·01-*D* Parker solutions containing respectively 7·47896 and 0·746253 g. of potassium chloride in 1000 g. of water. These solutions were assumed to have the new conductivity values as given by Jones and Bradshaw[5].

The thermostat consisted of a well-lagged container filled with liquid paraffin, and this could be rapidly heated by means of a 1200-watt Heatrae heater. To maintain any temperature two Ediswan lamps, each having a carrying capacity of 1 A., were employed, the current through them being operated by means of a Lowry spiral regulator filled with aniline. A rheostat in series with the lamps made it possible to adjust the heating-current to any desired value. With this arrangement the temperature was kept constant to within 0·01° C. over the whole range of temperature. The temperature was measured by means of a mercury-in-glass thermometer, recently calibrated at the National Physical Laboratory, which could be read to within 0·01° C. with the aid of a small microscope.

## § 3. RESULTS

It will be observed that the concentration range covered is from 0·01 to 0·001-*N*. As the temperature rises the concentration of the electrolyte between the electrodes decreases owing to the expansion of the solvent, and this fact must be taken into account when the equivalent conductances are calculated. The results are shown graphically in figure 1. For any given concentration the conductance increases with the temperature, the {conductivity, temperature} curves being slightly concave towards the axis of conductance.

It becomes of interest to see how far the Debye-Hückel-Onsager theory can account for the observed results. For a uni-bivalent electrolyte the Onsager equation assumes the form

$$\Lambda = \Lambda_0 - (\alpha\Lambda_0 + \beta)\sqrt{C},$$

in which $$\alpha = \frac{0{\cdot}986 \cdot 10^6 \, 4q\sqrt{3}}{(DT)^{\frac{3}{2}}(1+\sqrt{q})} \quad \text{and} \quad \beta = \frac{29{\cdot}0 \times 3\sqrt{3}}{(DT)^{\frac{1}{2}}\eta},$$

$q$ being given by $$q = \frac{2\Lambda_0}{3(\Lambda_0 + l)}.$$

In this equation $D$ is the dielectric constant, $T$ the absolute temperature, $\eta$ the viscosity of the solvent, $\Lambda$ the equivalent conductivity, $\Lambda_0$ the equivalent con-

ductivity at infinite dilution, $l$ the mobility of the univalent ion, and $C$ the concentration of the solution.

According to the theory, therefore, a plot of $\Lambda$ against $C^{\frac{1}{2}}$ should yield at the greatest dilutions a straight line whose slope should approximate to the value given by $(\alpha\Lambda_0+\beta)$.

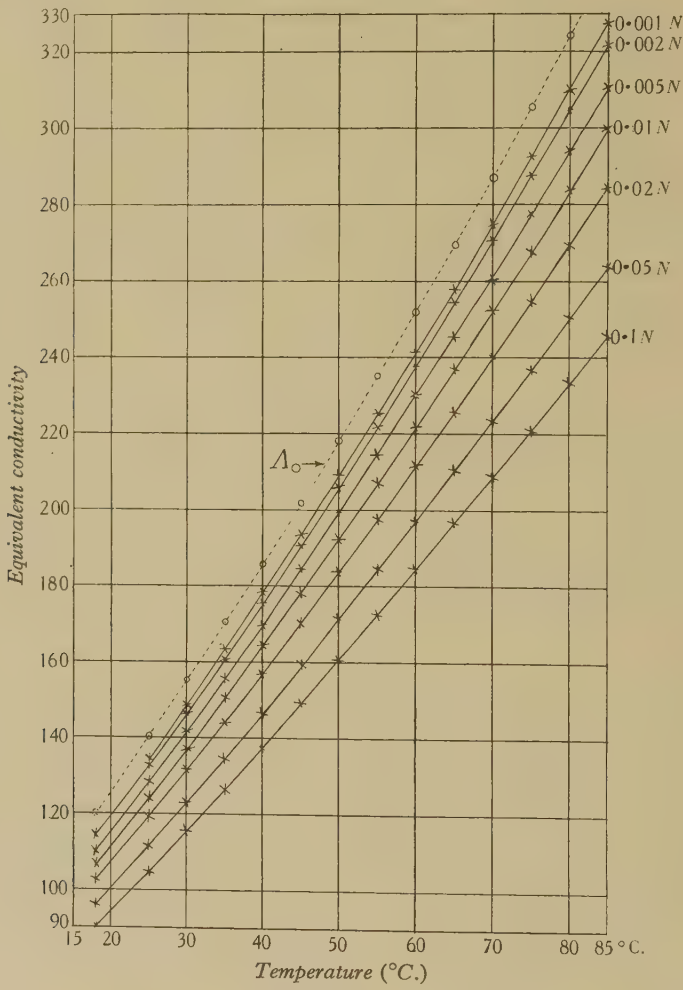


Figure 1.

Figures 2 and 3 show the plots of $\Lambda$ against $C^{\frac{1}{2}}$ for the present results. The curves do approach linearity as the concentration decreases and can therefore be extrapolated to give approximate values of $\Lambda_0$.

By employing Wyman's formula[6] for the variation of the dielectric constant of water with temperature, and consulting the *International Critical Tables* for the {viscosity, temperature} variation, the value of $\beta$ can be evaluated over the whole range 18–85° C. The calculation of the values of $\alpha$ over the same range

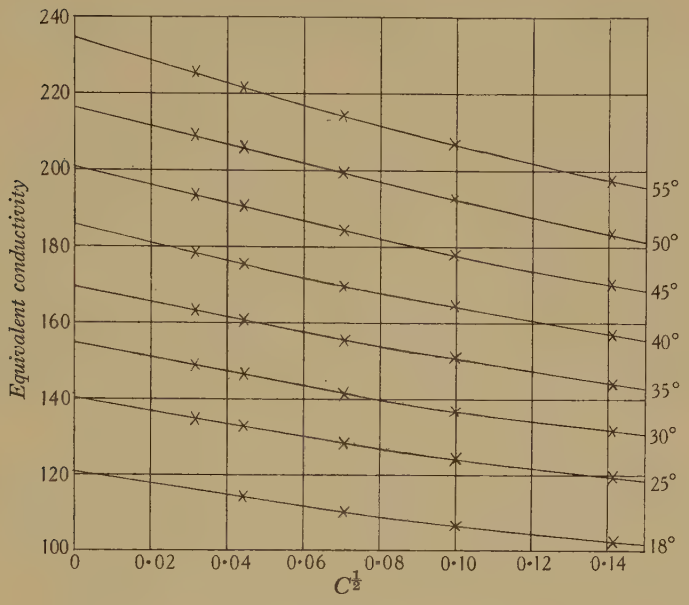


Figure 2.

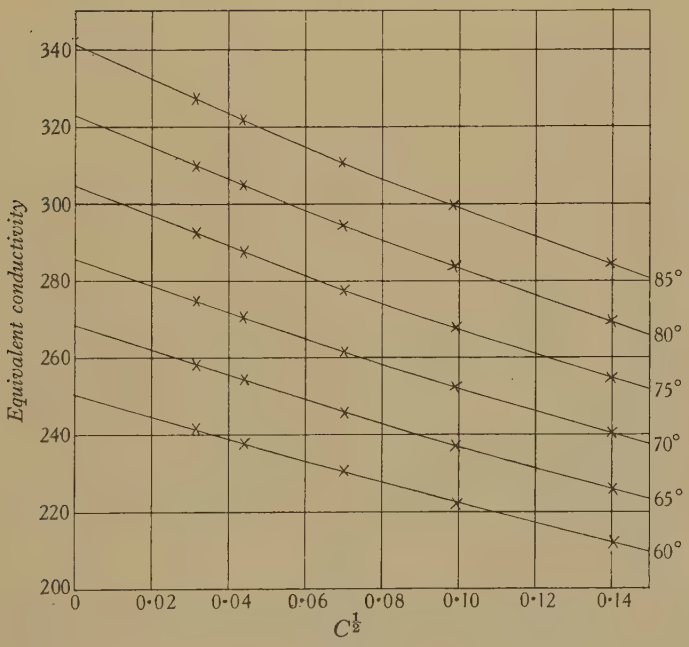


Figure 3.

involves the knowledge of the variation with temperature of the mobility of the chlorine ion. This can be found by plotting the values given in the Landolt Börnstein physico-chemical tables[7] and drawing a smooth curve through the points obtained.

Although the values thus found can only be approximate, this fact does not seriously detract from the accuracy of the test, since the term in which they occur, namely $q$, is insensitive to small variations in $l$. In fact, over the temperature range 18–85° C. the values of $q$ only varied between the limits 0·4321 and 0·4384.

In table 1 some calculated values for the slope of the $\Lambda - C^{\frac{1}{2}}$ curve are compared with the experimental values. While they are of the same order of magnitude it will be seen that in every case the experimental values are less than those calculated, so that Onsager's equation can only apply strictly at concentrations less than 0·001 $N$. That it does apply within this range, however, appears to be evident.

Table 1

| Temperature (°C.) | Theoretical slope | Experimental slope |
|---|---|---|
| 18 | 151·8 | 151·4 |
| 25 | 179·9 | 168·6 |
| 35 | 224·1 | 211·1 |
| 45 | 272·0 | 235·7 |
| 55 | 323·2 | 284·3 |
| 65 | 380·2 | 328·6 |
| 75 | 441·0 | 409·0 |

To determine the true $\Lambda_0$ values the method of Shedlovsky[8] was used, the calculated values of $\Lambda_0$, denoted by $\Lambda_0'$, being plotted against the concentration. For bi-univalent salts at 25° C. Shedlovsky found that the resultant plot was not quite linear[9], there being small systematic deviations at the lower concentrations, which, however, did not preclude the possibility of accurate extrapolation to zero concentration.

Figures 4 and 5 show the curves obtained from the present series of results. From 25 to 35° C. the curves retain the form found by Shedlovsky. Above this range, however, the plots become linear, and remain so over the range of temperature 40–85° C. At 18° C. deviations similar to those found at the other low temperatures exist, but in this case it was thought inadvisable to extrapolate to infinite dilution owing to the somewhat uncertain nature of the curve.

The values of $\Lambda_0$ found by this method are shown in table 2 and are not seriously different from those obtained from the $\{\Lambda, C^{\frac{1}{2}}\}$ curves. In fact, substitution of

Table 2

| Temperature (°C.) | 18 | 25 | 30 | 35 | 40 | 45 | 50 |
|---|---|---|---|---|---|---|---|
| $\Lambda_0$ | (119·9) | 140·1 | 155·0 | 170·1 | 185·6 | 201·7 | 218·0 |
| Temperature (°C.) | 55 | 60 | 65 | 70 | 75 | 80 | 85 |
| $\Lambda_0$ | 235·0 | 252·0 | 269·5 | 287·0 | 305·5 | 323·9 | 342·3 |

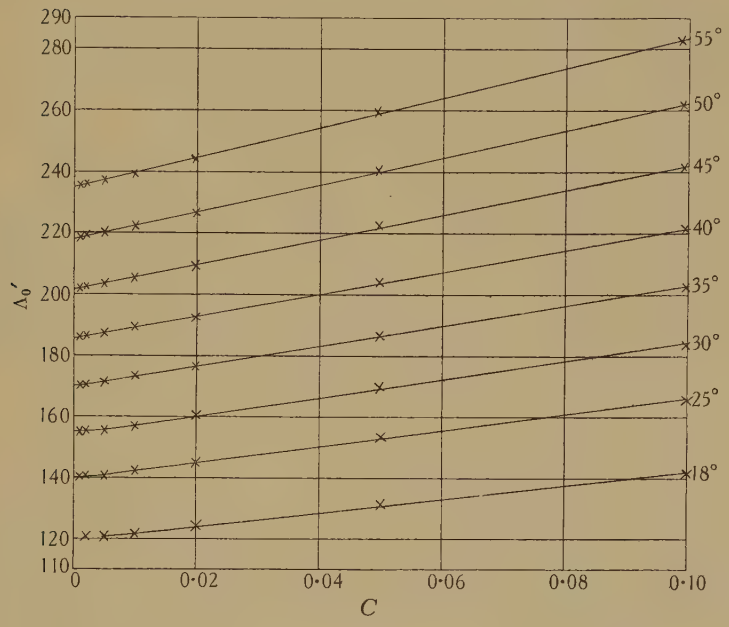


Figure 4.

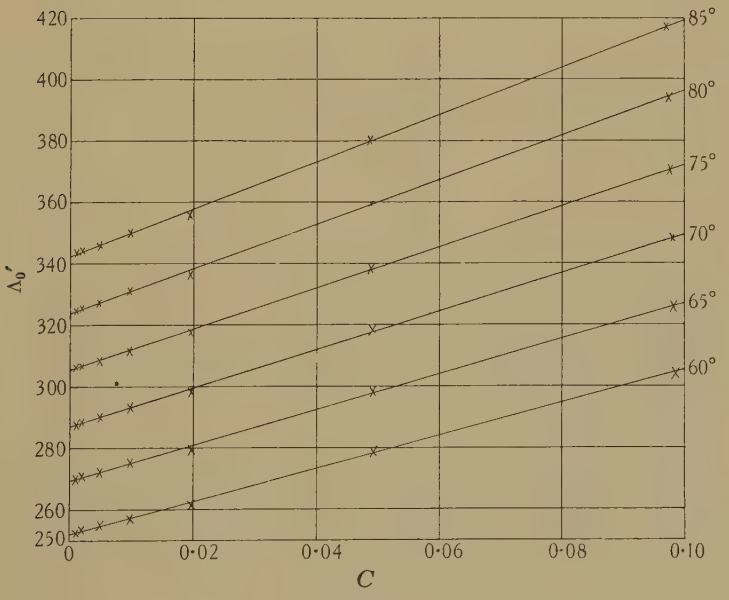


Figure 5.

these new $\Lambda_0$ values in the Onsager equation only slightly alters the $\Lambda_0'$ values at 0·1 and 0·05 $N$ and is inappreciable at the lower concentrations.

The influence of temperature on the equivalent conductivity at infinite dilution can be represented by an equation of the type

$$\Lambda_0{}^t = \Lambda_0{}^{25}\,[1 + a\,(t-25) + b\,(t-25)^2].$$

The constants $a$ and $b$ were evaluated by the method of least squares, the values found being respectively 0·02099 and 0·00005167. With these values for the constants the mean error introduced between the observed and the calculated values of $\Lambda_0{}^t$ over the temperature range 30–85° C. is of the order 0·06 per cent, so that for this wide temperature range the formula forms a very convenient and accurate method of representing the {conductivity, temperature} variation.

## § 4. CONCLUSION

The present investigation affords further evidence in support of the Debye-Hückel-Onsager theory in that it confirms the theoretical predictions as to the behaviour of dilute aqueous solutions of the bi-univalent electrolyte considered over a wide range of temperature. Further, it becomes evident from this work that an empirical extension of the theory previously thought to be applicable only to uni-univalent electrolytes is valid also for a bi-univalent salt at temperatures above those at which conductivity phenomena are usually studied.

## § 5. ACKNOWLEDGEMENTS

This work was carried out in the physical laboratories at Queen Mary College and I wish to tender my thanks to Prof. H. R. Robinson, F.R.S., for his interest and advice throughout the investigation.

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# THE IGNITION OF EXPLOSIVE GASES

By G. MOLE

*Communicated by Prof. G. I. Finch, March 27, 1936. Read October 23, 1936*

*ABSTRACT.* It is shown that the activation theory of ignition, when expressed mathematically, leads to a differential equation, the general ignition equation, which represents the state of an ignitible mixture. This equation yields solutions which agree closely with the fundamental phenomena of ignition.

## § 1. INTRODUCTION

Attempts to explain the mechanism of the ignition of explosive gaseous mixtures have proceeded along three lines. Taylor-Jones, Morgan and Wheeler[1,2,3,4] inclined to the view that ignition was a purely thermal process. They supposed that "the ignition of a gaseous mixture depends primarily ...on the heating of a sufficient volume to a sufficient temperature", from which followed as a natural corollary that "the heat energy required in the source for ignition is least when the heat is imparted instantaneously". On the other hand Thornton[5,6], to whom we owe the first proof of the significance of what he termed "the least igniting current", suggested that the role of the igniting source was to produce ions, the interaction of which led to ignition and subsequent flame-propagation maintained by ionization produced by the further chemical reaction. Finch and Thompson[10], however, showed by spectrographic means that the ignition and combustion of explosive mixtures of carbon monoxide and air were not conditioned by a prior ionization of any of the constituents, and they also demonstrated the inadequacy of the thermal theory to account for the facts when they showed that a decrease in discharge-frequency could completely outweigh the effect upon igniting power of a reduction in (i) the total energy, (ii) the rate of dissipation of energy or (iii) both, in ignition by either the first half-oscillation of a condensed discharge or by the whole discharge[13].

The results of further experiments by Bradford, Finch and Prior[14,15] in which explosive mixtures were ignited by coil discharges, showed that the capacity component of the discharge played a part subordinate to that of the inductance component in the process of ignition. They also found that the igniting power of the coil discharge was not a function of the total inductance-component energy alone but was also determined partly by the peak, and partly by the mean rate of dissipation to an extent which varied greatly with the nature of the gas mixture. Finally, they showed that, on thermodynamic grounds, the thermal theory was untenable because

it implied the view that heat was the only form of energy which could produ ignition, and that any other form of energy must be converted into heat befo becoming effective[15]. Such a view is in direct conflict with the well-known phen mena of the photo-ignition of hydrogen-chlorine mixtures and the discovery b Dixon and Croft[7] that dilution with oxygen in excess of the equivalent amou reduces the temperature of ignition of hydrogen-oxygen mixtures.

Finch and his collaborators[9,10,11,12,13,16,17] have put forward the view th ignition and self-propellant combustion are determined by the building up of sufficient concentration of suitably excited molecules. The igniting energy is r garded as the source of suitable excitation in the case of ignition, and the energy s free as a result of the chemical action between excited molecules as the source supply of the excited molecules which maintain the subsequent independent com bustion. This view is now generally known as the *activation theory of ignition.* has so far been expressed only in qualitative form, i.e. "ignition is primarily dete mined by the setting up of a sufficient concentration of suitably activated mol cules"[13].

There are striking similarities between the behaviour of activated molecul taking part in ignition and gaseous ions undergoing recombination. Thus it woul seem that the method successfully applied by Langevin[20] to the quantitativ analysis of the phenomena of the recombination of ions might well be adapted to quantitative study of the mechanism of ignition. This has been attempted in wha follows.

## § 2. GENERAL IGNITION EQUATION

$t, n, n'$ Consider the element of gaseous mixture which is the locus of ignition. Let th numbers of suitably activated molecules per unit volume at any time $t$ be $n$ and $n'$ Let the rates of formation of suitably activated molecules per unit volume at time $q, q', p$ be $q$ and $q'$. Let the rate of combination per unit volume at time $t$ be $p$ and th $s, s', \alpha$ rates of deactivation per unit volume $s$ and $s'$. Then, if $\alpha$ is the coefficient of com bination,

$$p = \alpha n n' \qquad \text{.....(1).}$$

The general equations of state are

$$\left.\begin{aligned} \frac{dn}{dt} &= q - p - s \\ \frac{dn'}{dt} &= q' - p - s' \end{aligned}\right\} \qquad \text{.....(2).}$$

and

$m_1, m_1'$ Let the rates of activation be $m_1$ and $m_1'$ per unit of energy dissipated by the $m_2, m_2'$ igniting agent and $m_2$ and $m_2'$ per unit of energy liberated on combination. Suppose $R$ further that the rate of energy-dissipation by the igniting agent at time $t$ is $R$ $j$ and that the energy liberated as the result of one combination is $j$. Then,

$$\left.\begin{aligned} q &= m_1 R + m_2 jp \\ q' &= m_1' R + m_2' jp \end{aligned}\right\} \qquad \text{.....(3).}$$

and

Eliminating $q$ and $q'$ from equations (2) and (3) we have

$$\left.\begin{aligned}\frac{dn}{dt} &= m_1 R + (m_2 j - 1)\, p - s \\ \text{and} \quad \frac{dn'}{dt} &= m_1' R + (m_2' j - 1)\, p - s'\end{aligned}\right\},$$

which may be written

$$\left.\begin{aligned}\frac{dn}{dt} &= m_1 R + \alpha\,(m_2 j - 1)\, nn' - n/\omega \\ \text{and} \quad \frac{dn'}{dt} &= m_1' R + \alpha\,(m_2' j - 1)\, nn' - n'/\omega'\end{aligned}\right\} \qquad \text{.....(4),}$$

where $\omega$ and $\omega'$ are the average lives of activated molecules which do not combine. $\omega, \omega'$

The supply of explosive mixture may be regarded in practice as inexhaustible. Thus $m_1$, $m_2$, $\omega$ and $j$ are constant with respect to time. Let the igniting agent be such that $R$ is also constant with respect to time. Equation (4) may then be written:

$$\left.\begin{aligned}\frac{dn}{dt} &= a + bnn' - cn \\ \text{and} \quad \frac{dn'}{dt} &= a' + b'nn' - c'n'\end{aligned}\right\} \qquad \text{.....(5),}$$

where $a$, $b$, $c$ and $a'$, $b'$, $c'$ are positive constants the values of which are given by $a, b, c, a', b', c'$

$$\begin{aligned} a &= m_1 R, & a' &= m_1' R, \\ b &= \alpha\,(m_2 j - 1), & b' &= \alpha\,(m_2' j - 1), \\ c &= 1/\omega, & c' &= 1/\omega'. \end{aligned}$$

These are simultaneous non-linear differential equations which are easily solved when $n = n'$. Thus,

$$\frac{dn}{dt} = a + bn^2 - cn \qquad \text{.....(6).}$$

The solutions are the following.

*Case I.* $a = 0$.

The complete solution is

$$n = c/b\,\{1 + (c/bn_0 - 1)\, e^{ct}\} \qquad \text{.....(7),}$$

where $n_0$ is the value of $n$ when $t = 0$. The curve to equation (7) is drawn in figure 1. $n_0$

*Case II.* $a < c^2/4b$.

The complete solution is

$$n = \frac{\phi_1 (n_0 - \phi_2)\, e^{\phi_2 bt} - \phi_2 (n_0 - \phi_1)\, e^{\phi_1 bt}}{(n_0 - \phi_2)\, e^{\phi_2 bt} - (n_0 - \phi_1)\, e^{\phi_1 bt}} \qquad \text{.....(8),}$$

where $\phi_1$ and $\phi_2$ are the roots of the quadratic $\phi_1, \phi_2$

$$bn^2 - cn + a = 0.$$

The curve to equation (8) is given in figure 2.

*Case III.* $a=c^2/4b$.

The complete solution is

$$\left.\begin{aligned} n&=c/2b-1/\{bt+2/(c-2bn_0)\}\\ t&=2/(c-2bn)-2/(c-2bn_0)\end{aligned}\right\} \quad \text{.....(9).}$$

or

The curve to equation (9) is given in figure 3.

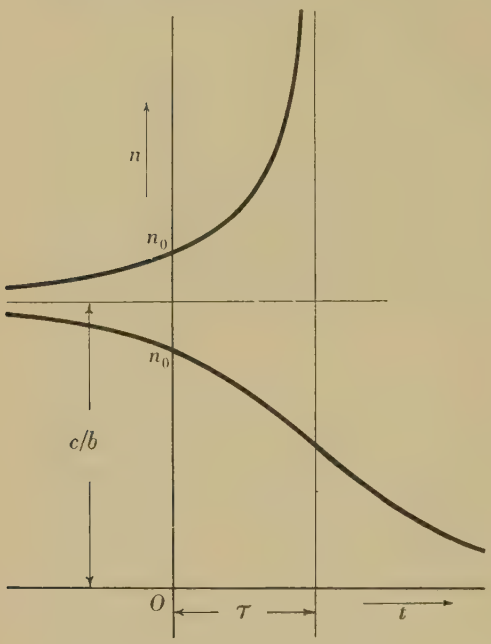


Figure 1. $a=0$. $\tau=\frac{1}{c}\log\frac{bn_0}{bn_0-c}$.

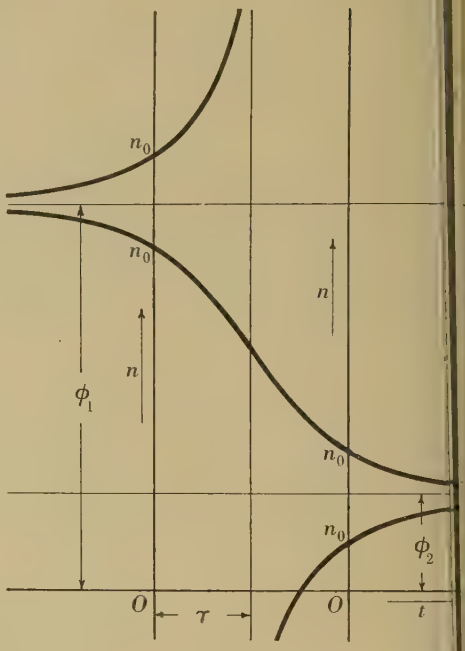


Figure 2. $a<c^2/4b$. $\tau=\frac{1}{b(\phi_1-\phi_2)}\log\frac{n_0}{n_0}$

$\phi_1=\{c+\sqrt{(c^2-4ab)}\}/2b$; $\phi_2=\{c-\sqrt{(c^2-4ab)}$

*Case IV.* $a>c^2/4b$.

The complete solution is

$$\left.\begin{aligned} n&=\frac{1}{2b}\left[c+\gamma\tan\left\{\frac{\gamma t}{2}+\tan^{-1}\frac{2bn_0-c}{\gamma}\right\}\right]\\ \frac{\gamma t}{2}&=\tan^{-1}\frac{2bn-c}{\gamma}-\tan^{-1}\frac{2bn_0-c}{\gamma}\end{aligned}\right\} \quad \text{.....(10),}$$

or

$\gamma$ where $\gamma=\sqrt{(4ab-c^2)}$.

The curve to equation (10) is given in figure 4.

Thus four distinct types of solution to the general ignition equation are obtain able. The solutions are dependent on the relative values of the quantities $a$ an $c^2/4b$. The condition for self-propagation, $a=0$, leads to the solution of case (figure 1). From this it is seen that the initial activation $n_0$ has a critical value $c/b$ below which the concentration of activated molecules $n$ drops to zero as $t$ approache infinity and above which $n$ proceeds to infinity in the finite time

$\tau$ $$\tau=c^{-1}\log\{bn_0/(bn_0-c)\}.$$

This shows that an ignitible mixture, if free from any source of activation, should

remain in stable equilibrium without combustion taking place, but that if it is activated beyond a certain critical value it should explode. This is the most fundamental phenomenon which must be explained by any theory of ignition.

If the intensity of the source of activation is less than the critical value given by $a=c^2/4b$ then the equations of case II (figure 2) apply. Under these conditions it is seen that an explosion will occur only if $n_0$ possesses a value higher than $\phi_1$. The critical value $\phi_1$ is however less than the corresponding value $c/b$ for self-propagation. For lower values of $n_0$ the concentration of activated molecules approaches the steady value $\phi_2$ and remains at that value so long as the activating

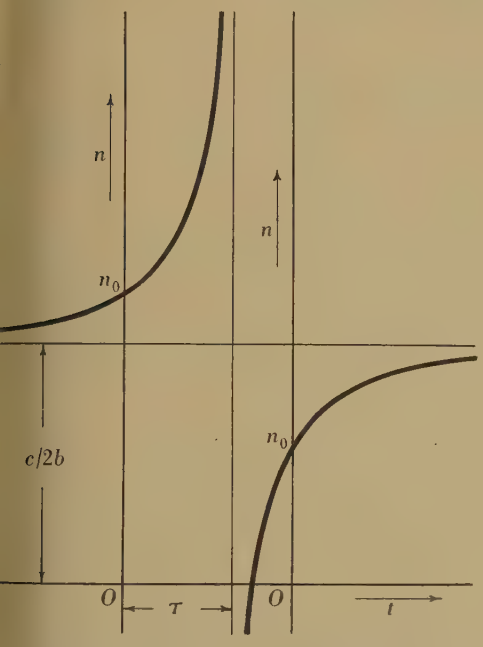


Figure 3. $a=c^2/4b$. $\tau=2/(2bn_0-c)$.

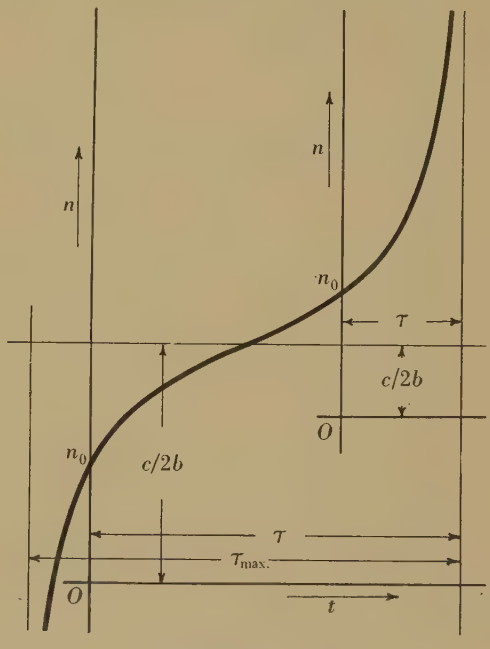


Figure 4. $a>c^2/4b$.
$\tau=\frac{1}{\gamma}\left(\pi-2\tan^{-1}\frac{2bn_0-c}{\gamma}\right)$; $\tau_{\max.}=2\pi/\gamma$.

source is maintained. Slow combustion is thus to be expected and should occur at a steady rate given by $p=\alpha\phi_2^2$. Such slow combustion has often been demonstrated in heated gases, in electric discharges[8, 10, 11, 12, 16] and in mixtures under the influence of photoactive radiation.

When the intensity of the source of activation is greater than the critical value given by $a=c^2/4b$ the equations of case IV (figure 4) apply. Under these conditions the mixture must always explode in the finite time given by

$$\tau=\frac{1}{\gamma}\left(\pi-2\tan^{-1}\frac{2bn_0-c}{\gamma}\right).$$

The time taken to explode can never exceed the value $\tau_{\max.}=2\pi/\gamma$. The critical condition for ignition by a continuous source is thus $a=c^2/4b$. The condition for $\tau_{\max.}$

ignition by a source of limited duration, obtained by substituting $n_0=0$ and $n=c$ in equation (10), is $c=\gamma \tan(\gamma\tau/4)$.

The theory yields one more critical condition. If the coefficient $b$ is negative i.e. if the value of the quantity $m_2 j$ is less than unity, then the term $\gamma$ in equation (10) becomes imaginary and equation (7) becomes

$$n=c/b\,\{(1+c/bn_0)\,e^{ct}-1\},$$

from which it will be seen that $n$ approaches zero for all positive values of $t$. The mixture will therefore neither explode under the influence of an activating agent nor propagate an explosion. It follows that $m_2 j=1$ is the condition which determines the limits of inflammability of an explosive mixture.

## § 3. SLOW COMBUSTION

The rate of slow combustion per unit volume at any point in the mixture is given in molecules per unit time by

$$p=\alpha\phi_2{}^2=\{1-\sqrt{(1-4ab/c^2)}\}^2\,\alpha c^2/4b^2.$$

The rate of slow combustion for the whole mixture or for any part of it is given by

$C$ $$C=\int\frac{\alpha c^2}{4b^2}\{1-\sqrt{(1-4ab/c^2)}\}^2\,dV \qquad \text{.....(11),}$$

$V$ where $V$ refers to volume.

## § 4. IGNITION BY ELECTRICAL DISCHARGE

It is possible to make a theoretical comparison of the relative igniting properties of high-frequency and low-frequency discharges. Suppose the discharge is continuous and possesses the energy-dissipation wave-form shown in figure 5. During the time intervals 1–2 the equations of case IV apply, and during the time intervals 2–1 the equations of case I apply. The variation of $n$ with $t$ for least igniting conditions will be as shown in figure 6. An examination of the shapes of the curves

$n_1, n_2$ shows that the equilibrium is stable for values of $n_1+n_2$ less than $c/b$ but unstable for values greater than $c/b$. The relation $n_1+n_2=c/b$ is therefore the condition for ignition. The following equations are then true:

$$\frac{\gamma\tau}{2}=\tan^{-1}\left(\frac{2bn_2-c}{\gamma}\right)-\tan^{-1}\left(\frac{2bn_1-c}{\gamma}\right),$$

$$n_1=c/b\,\{1+(c/bn_2-1)\,e^{c\tau}\},$$

and $$c/b=n_1+n_2.$$

Eliminating $n_1$ and $n_2$,

$$\tanh\frac{c\tau}{4}=\frac{\gamma}{c}\tan\frac{\gamma\tau}{4} \qquad \text{.....(12).}$$

Substituting $\gamma=\sqrt{(4ab-c^2)}$,

$$\tanh\frac{c\tau}{4}=\sqrt{\left(\frac{4ab}{c^2}-1\right)}\tan\left\{\frac{c\tau}{4}\sqrt{\left(\frac{4ab}{c^2}-1\right)}\right\}.$$

Approximating,

$$\tanh\frac{c\tau}{4}=\left(\frac{4ab}{c^2}-1\right)\frac{c\tau}{4} \qquad \text{.....(13).}$$

For values of $c\tau/4$ small compared with unity, equation (13) reduces to

$$a = c^2/2b.$$

When $c\tau/4$ is large compared with unity, equation (13) reduces to

$$a = c^2/4b.$$

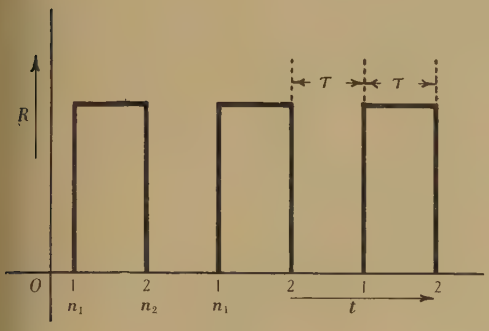


Figure 5.

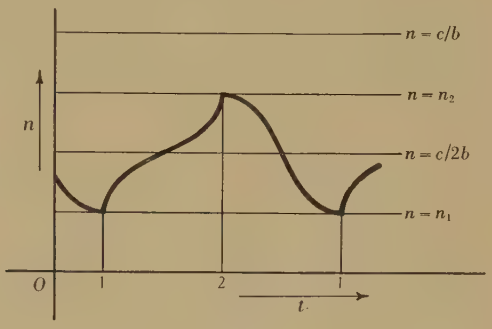


Figure 6.

It follows that the variation of $a$ with frequency should be as shown in figure 7 and that the igniting power of a high-frequency discharge should be approximately half that of a low-frequency discharge of similar energy. This is found to be true in practice. Finch and Thompson's[13] results for mixtures of carbon monoxide with air are shown graphically in figure 8.

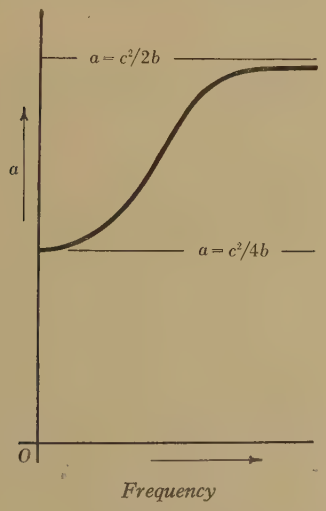


Figure 7.

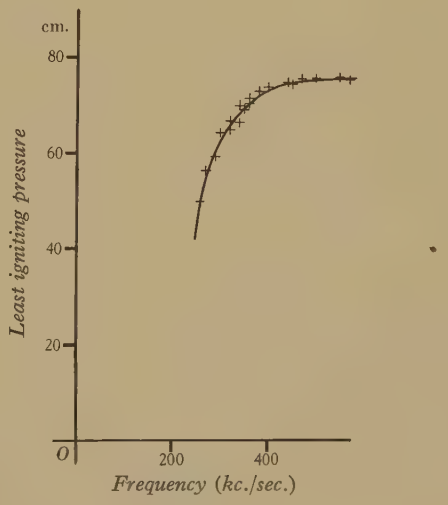


Figure 8.

Equation (13) can be used in conjunction with the experimental results of figure 8 to calculate an approximate value for $\omega$, the average life of an activated molecule. At the point where $a \doteqdot 1{\cdot}7c^2/4b$, equation (13) gives $c\tau/4 = 1$ or $\omega = 1/8f$, where $f$ is the frequency of the discharge. For carbon monoxide at atmospheric pressure the life of an activated molecule suitably excited for ignition should there-

fore be of the order of 0·4 microsecond. The lives of activated molecules in meta stable states of excitation are well known to be of this order[18,19].

## § 5. ACKNOWLEDGEMENT

The author wishes to record his high appreciation of the experience gained fro work carried out in the electrochemistry laboratories of the Imperial College o Science and Technology, and to thank Prof. G. I. Finch for his valuable assistanc in connexion with the subject of the paper.

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# A SELF-SYNCHRONIZING TIME BASE FOR THE DISTANT OBSERVATION OF WIRELESS PULSE TRANSMISSIONS

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*ABSTRACT.* A circuit is described for producing in a cathode-ray oscillograph a time base suitable for the investigation of wireless pulses reflected from the ionosphere when no ground wave is present. The time base is locked to the received groups of echoes and gives a stationary pattern on the oscillograph even when all the echoes are fading considerably. The whole group of echoes, including the first, is delineated on a portion of the time base which is not obscured by end effects in the trace, so that accurate measurements of time intervals can be made.

## § 1. INTRODUCTION

In using pulse-transmissions of the Breit and Tuve type for the study of the ionosphere it has been usual to place the receiving station close to the transmitter, so that the received wave consisted of a strong ground wave followed by one or more echoes from the reflecting regions. Under these conditions the time base of the oscillograph can be readily synchronized with the recurrence frequency of the transmitted pulses either by operating both from the same a.c. supply mains [1], or else by making use of the constant-amplitude ground wave as a synchronizing impulse [2]. When it is desired to observe pulses returned from the ionosphere to a distant receiving station these methods of synchronizing cannot be employed, since the supply mains at two widely separated places always exhibit some slight drift of phase and, further, for all but the smallest distances the ground wave is too weak to be received. A modified form of time base has therefore been developed for distant working, the requirements for which may be summarized as follows. (1) The time-base traverse should be locked to the received groups of echoes (there being more than one echo in general for each transmitted pulse) so as to give a stationary pattern on the oscillograph, even when all the echoes are fading considerably. (2) In order that relative times of arrival may be measured accurately for the several echoes, the whole group including the first, perhaps weak, echo should be delineated on a portion of the time base which is not obscured by end effects in the trace.

The apparatus described below, which is based on a design due to Scholz[3], has been found to fulfil these two conditions well, and with it measurements of the time spacing between the arrival of echoes at a distant station have been made with considerable accuracy.

## § 2. THE CIRCUIT ARRANGEMENT

The transmitted pulses were sent at a recurrence frequency of 50 c./sec. and the method employed to ensure that the first echo should not be obscured at the end of the oscillograph trace was to operate the receiver time base at a repetition frequency of 25 c./sec., thus allowing one group of echoes to be used for starting the time base while the next appeared, in a convenient form for measurement, on an undistorted part of the trace.

A thyratron was employed for controlling the time base, the circuit details of which are shown in figure 1. The diode detector of the receiving amplifier is wired to give a positive voltage output relative to earth and this is fed to the grid of valve $V_1$, the anode of which provides the echo voltage for the vertical deflecting plates of the oscillograph. Part of this same potential is passed to the grid of $V_2$ in

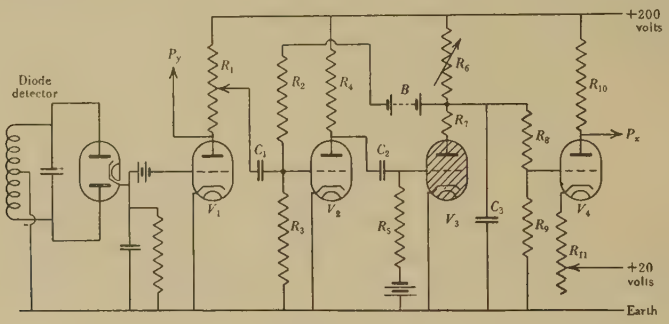


Figure 1.

the form of negative impulses, and from there to the grid of the thyratron $V_3$, where it appears as positive impulses. The thyratron is arranged as a relaxation oscillator by virtue of the condenser and resistance $C_3$ and $R_6$, and its frequency is adjusted to be close to 25 c./sec. Its anode-potential therefore rises gradually from the extinguishing potential of about 15 V. to the striking potential (which is determined by its grid bias) of about 100 V. Half of this potential-variation is applied through the biasing battery $B$ to the grid of the previous valve $V_2$, where it has the effect of desensitizing this valve for all but the last part of the potential-rise. Since the signal voltage impulses on the grid of $V_2$ are negative in relation to earth, it is clear that during the desensitized period they can have no effect at its anode, however strong the echoes may be. The battery voltage $B$ is made equal to about 90 V., so that only when the condenser $C_3$ has charged up to close on this value will the valve $V_2$ begin passing current. Immediately it becomes conducting, the impulses on its grid appear as positive pulses at its anode, and the first of these, unless extremely weak, will cause the thyratron to strike. In this way the thyratron

oscillation is made to lock stably to alternate groups of echoes even when the relative amplitudes of the different echoes are varying considerably with time.

The anode-potential of the thyratron does not provide a suitable time base for observing the alternate groups of echoes since the whole of each group arrives within a voltage-rise of only about 10 V., occurring when the potential difference across $C_3$ is at about half its final value. It is necessary therefore to amplify this portion alone of the time-base, so that the echo pattern can be made to cover a reasonably large portion of the oscillograph screen. This is done by means of the valve $V_4$, which has a variable positive voltage of about 20 V. on its cathode. It therefore begins to conduct only when the voltage on its grid approaches this value, i.e. when the condenser-voltage approaches twice this value, since the resistances $R_8$ and $R_9$ are equal in magnitude. After this the current in the valve rises rapidly to saturation, and then remains constant for the rest of the voltage-rise. The anode-potential of this valve provides the horizontal deflection on the oscillograph, and the magnitude of the voltage-rise on $C_3$ which is embraced by its active period is controlled by a cathode resistance $R_{11}$.

Although the rise of voltage across $C_3$ occurs exponentially with time, the portion, extending over about 10 V., which is amplified by $V_4$ is almost linear, so that by choosing a good triode for this amplifier the resulting time base can be made very nearly linear. It will be seen that the curvatures introduced by the exponential rise and by non-linearity in the valve characteristic are opposing, and it was found in practice that the second was the more important. It was found, however, that by connecting a shunt resistance across the condenser, and so exaggerating slightly the exponential effect, the two curvatures could be made to cancel with considerable precision, and thus a time base which was linear over its whole range within the accuracy of measurement was obtained. The resistance used for this, not shown in the figure, was 50,000 Ω.

Care was necessary in choosing values for the condenser and resistance coupling between $V_1$ and $V_2$ in order that the desensitizing bias should be applied effectively to $V_2$ through the grid resistance $R_2$, while the pulses were passed on from the anode of $V_1$ without serious loss of amplitude. A suitable time constant for the grid leak and condenser was found to be 0·0025 sec.

The values used for the important components in the apparatus were as follows:

$R_1$, 50,000Ω.; $R_2$, 1 MΩ.; $R_3$, 1 MΩ.; $R_4$, 50,000Ω.; $R_5$, 100,000Ω.; $R_6$, 100,000Ω.; $R_7$, 300 Ω.; $R_8$, 100,000 Ω.; $R_9$, 100,000 Ω.; $R_{10}$, 80,000 Ω.; $R_{11}$, 10,000 Ω.; $C_1$, 0·005 μF.; $C_2$, 0·001 μF.; $C_3$, 0·6 μF.

Both the receiving amplifier and the time-base apparatus incorporated a.c. mains-operated valves, and the high-tension supply was obtained from a rectifier. It is of interest, therefore, to note that the circuit showed no tendency to lock to the local frequency instead of to the received pulses.

A 750-c./sec. oscillator was used for calibration of the time-base speed, the oscillation being applied in place of the signal voltages to the oscillograph when required. In order that a stationary curve should be formed on the screen the

oscillator had to be locked to the signal-recurrence frequency, and this was accomplished by using, as a dynatron oscillator, a screen-grid valve the control grid of which was loosely coupled to the thyratron anode. The sudden discharges of the latter provided a sufficiently strong 30th harmonic to ensure stable locking, and the method of electron coupling avoided any reaction back from the oscillator to the time-base circuits.

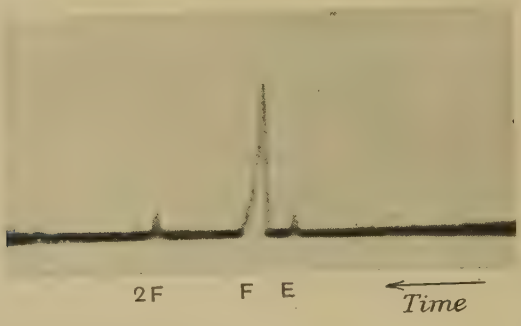


Figure 2.

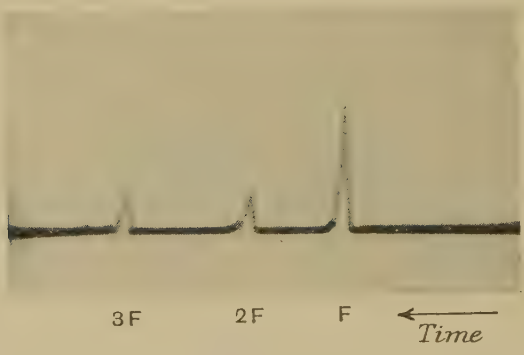


Figure 3.

The apparatus has been used in a number of experiments on the oblique reflection of waves from the ionosphere, an account of which is being published elsewhere. Figures 2 and 3 show two typical echo patterns obtained with the receiving apparatus at Cambridge, with in this case a distant (unknown) transmission on a wave-length of 80 m. There is no ground wave, and it is seen that in the first picture a weak *E* echo is present together with the first- and second-order *F* reflections, while in the second only *F* echoes are received.

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# A PHOTOELECTRIC SPECTROPHOTOMETER OF HIGH ACCURACY

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*Communicated by H. F. Buckley, June* 8, 1936. *Read in title October* 23, 1936.

*ABSTRACT.* Details are given concerning the components used in constructing a photoelectric spectrophotometer employing as light-sensitive element a vacuum emission photocell of a modern type with central plate cathode. It is claimed that the instrument as a whole has a linear response over the necessary range of photocurrents and the method by which this property was tested is described. The instrument has been applied to the measurement of spectral transmissions of the International Standard Blue Filter R 3.28 and the results, which are in good agreement with the internationally adopted figures, are given in full. The effect of temperature on this filter is shown also.

## § 1. INTRODUCTION

Until recently attempts to use the emission photocell as a direct measuring instrument have not met with entire sucess owing to the fact that even if stability was ensured by the use of vacuum cells, these cells frequently did not give a linear response to variable illumination. Various methods of avoiding this and other possible defects in the photocell have been tried, depending generally on the use of the photocell merely to obtain a balance between two illuminations (usually of identical spectral composition), instead of to measure one of them directly in terms of photocurrent. These methods, while having very definite advantages in eliminating the effects of undesirable features in the photocell, have other practical disadvantages, optical and electrical.

However, it has now become possible to obtain emission photocells of the vacuum type which have a sensibly linear {illumination, current} characteristic, at least in the region of photocurrents likely to be concerned in the case of spectrophotometry, say from $5.10^{-10}$ A. downward. Thus, while from the purely philosophical point of view methods of spectrophotometry such as those already mentioned may be the more fundamental, the direct use of the photocell as the measuring instrument nevertheless becomes possible. Even if a complicated electrical circuit still remains necessary, reliability and simplicity of operation then show a marked gain, as compared with the indirect methods.

The features chiefly desired in a direct-reading photoelectric spectrophotometer are (1) linear performance of the instrument as a whole, (2) obedience to Talbot's

law if possible, (3) good sensitivity, (4) electrical and mechanical stability, and (5) avoidance of stray light in the beam incident on the test filter. The second feature may be dispensed with provided a rigorous demonstration of the first is possible.

The object of the present paper is to describe a spectrophotometer which satisfies at least the requirements (1), (3), (4) and (5), and the method by which its linearity was tested at any desired wave-length. Results obtained with it are quoted and compared with those obtained by other methods. No novelty is claimed in respect of either the optical or the electrical principles embodied in the instrument, but since its satisfactory assembly and operation depend almost entirely on strict attention to technical details, these are fully set out.

## § 2. THE OPTICAL SYSTEM

*The monochromator.* The optical system is shown in figure 1. The nucleus of the system is an Adam Hilger constant-deviation spectrometer of aperture about F/11. In the instrument used by the authors the telescope slit is symmetrical, while the collimator slit is not. For convenience, both slits should be symmetrical.

*The source of light.* The source of light $S$ is a Siemens 12-volt 48-watt gas-filled lamp of commercial type, with a single-pillar coiled filament having no supports other than the leading-in wires. The lamp is burned cap down. This lamp may be run for long periods at 13·5 V. It was found more satisfactory as regards available light and steadiness than either a gas-filled strip-filament lamp, or other lamps of the projection type. Moreover, the filament is stout enough not to become distorted appreciably over moderate periods of time, even if run at 13·5 V.

*The condensing lens.* The system used to focus an image of the light-source on the collimator slit need not have an aperture greater than that of the spectrometer. To avoid loss of light by reflection the number of glass surfaces should be a minimum. In practice an achromatic doublet $L_1$ of focal length about 10 cm. and diameter 3·5 cm. is used, the distance from lens to lamp filament being 17 cm. and that from lens to slit 25 cm. Appropriate light screens are used and are shown in figure 1. The image of the lamp filament should, of course, be larger than the collimator slit to obtain the maximum available light.

*The mechanical supports.* Since absolute rigidity of the optical system is essential to maintain constant the light entering the system, the lens and light source are mounted on a piece of stout aluminium of L-section attached to the spectrometer bedplate. The spectrometer and also the remainder of the optical and electrical systems mentioned below, except batteries, galvanometer, and potentiometer, are clamped down to a slate slab.

*The exit lens.* The lens $L_2$ is used to receive the light emerging from the telescope slit and to render it parallel for transmission through the test filter. Its focal length may suitably be 15 cm. and its aperture at least that of the spectrometer system. It is placed at its focal length from the telescope slit and thus throws light on an area of the photocell cathode of shape similar to the section of the light beam

entering the telescope lens. By suitable choice of the focal length of this lens this illuminated area is made nearly large enough to extend to the edges of the cathode.

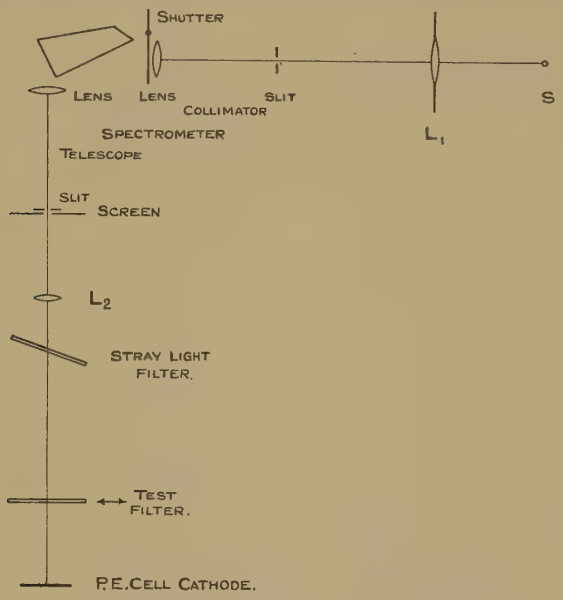


Figure 1. Diagrammatic plan of optical system.

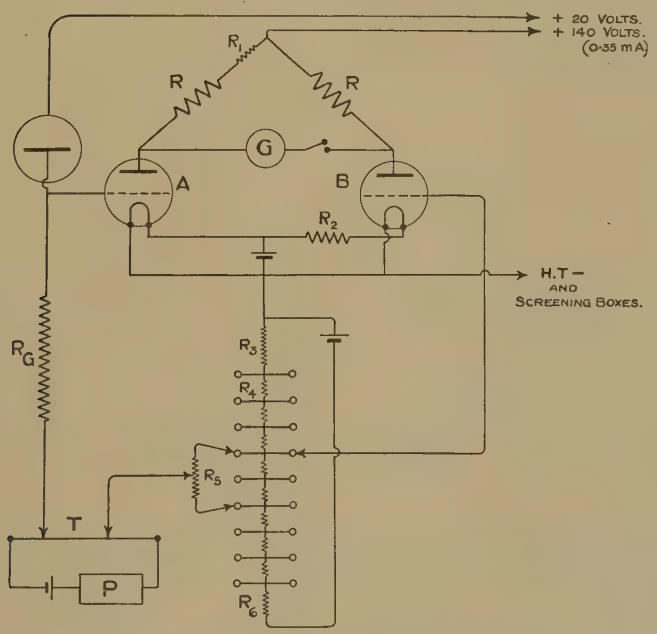


Figure 2. Electrical circuit.

The exit lens is tilted very slightly from the normal position so as to avoid inter-reflections between the components of the system.

The reasons for adopting this exit system are that it is simple, as regards number of lenses required, and absorption is consequently low; it gives a patch of light on the cell cathode large enough to minimize any errors due to slight displacement of the patch over areas of varying sensitivity, which displacement may possibly occur when a thick or slightly wedge-shaped test filter is in use; and it gives minimum divergence of the light traversing the test filter, and hence minimum error due to obliquity of the rays traversing this filter.

*The stray-light filter.* This filter is used to remove stray light of unwanted wavelengths from the exit beam, and is chosen therefore to have a principal transmission band in the wave-length region where a measurement is being undertaken, and high absorption elsewhere. A set of coloured glasses such as Chance's is useful from which to choose a stray-light filter. The filter is placed in a holder 3 or 4 cm. from the exit lens as shown in figure 1 and is inclined to the optical axis to avoid inter-reflection with other components of the system.

*The test filter.* The test filter is placed in a holder which is movable so that the filter may be inserted in or removed from the light beam at will. Care should be taken that this filter shall be also slightly inclined to the axis of the system, to avoid the formation of a strong reflected image of the slit on the metal work of the slit. This image should be displaced on to the black screen shown in figure 1 and so absorbed. The position for the test filter may suitably be about 8–10 cm. behind the stray-light filter and say 12–15 cm. from the photocell.

*The photocell.* The final component of the optical system is the photocell. The type of cell chosen was an Osram KMV 6. This cell has the advantages that the cathode is an easily visible metal plate, and that its spectral sensitivity makes it suitable for measurements throughout the visible spectrum. It has also been found that selected cells of this type are among the best for linearity of characteristic, and that they have good electrical stability.

*General remarks.* The optical system just described and shown diagrammatically in figure 1 was chosen to secure a maximum of available light. It is believed that all the precautions necessary to exclude the most common sources of error have been taken. It is to be remembered, however, that some sources of error may still arise. For instance the interposition of a thick filter in the exit beam may alter slightly the size of the illuminated area of the photocell cathode. If the cathode is not uniformly sensitive some error may accrue, but it is likely to be much less than if the filter were inserted in the light beam on the collimator side of the spectrometer, with possible displacement of the image of the lamp filament on the collimator slit.

The condensing lens must be achromatic and it is an advantage as regards focusing if the exit lens also is achromatic. Too many glass-air interfaces seriously reduce the available light, and for this reason complicated lens systems are undesirable.

Temperature-control may be necessary for the measurement of filters of certain classes, but is not dealt with here. Change in temperature of the test filter due to absorbed radiation will not, however, be so serious in the present instrument as in

cases where the test filter is interposed in the more intense beam of light between the source and the collimator.

Finally, it is advisable so to design the system that no outside illumination falls on the photocell during the shifting of filters. Insertion of the test filter and the opening and closing of the shutter on the spectrometer is conveniently done with Bowden wire controls. Familiarity with the necessary precautions in the calibration and use of the spectrometer is assumed throughout this paper.

## § 3. THE ELECTRICAL SYSTEM

*The circuit.* This is shown diagrammatically in figure 2. It is the well-known valve-bridge circuit and differs in no essentials from that used by previous workers[1, 2]. Compensation of both filament and anode circuits is provided for, to render the balance of the bridge independent of variations of battery voltage. Control of the voltage on both grids over a small range is also possible, apart from the separate voltage-control used on the grid connected with the photocell for purposes of measurement.

*Thermionic valve unit.* The valves, of Mullard type PM1A, were supplied as a matched pair by the makers. The wisdom of using dull-emitter valves of general-service type in a set-up of high sensitivity may be doubted, but the authors believe that the circuit described here is as stable as a circuit of comparable sensitivity using pliotrons or electrometer valves. Good amplification is obtained with a comparatively low grid resistance, while the total anode current is less than 0·4 mA. The main difference is that the poorer grid insulation in the case of the PM1A valve makes it unsuitable for a rate-of-charge method. The deflection sensitivity is however at least as great as that of the circuit described by Dubridge[3], in which pliotrons were used (see Appendix).

The two PM1A valves are uncapped and fixed in holes in a wax block, the cap end projecting uppermost, by melting wax round the tops of the bulbs. By the use of stiff wire and terminals melted into the sides of the wax block, the resistors $R$, $R$, $R_1$ and $R_2$ are made independent of any other support. Six terminals are used and serve respectively for the filament supply (two), the galvanometer leads (two), the h.-t. supply (one), and the grid control on valve $B$ (one).

*Anode resistors.* The anode resistors $R$, each of 700,000 ohms, are Karbowid resistors, type 2*b*, supplied by Messrs Siemens-Schuckert. Though not wire-wound these resistors are stable and have a low temperature coefficient.

*Compensating resistors.* The resistor $R_1$ may well be a Karbowid resistor of suitable value, determined as described below. It may have to be inserted in either anode circuit according to the direction in which compensation is found to be required. $R_2$ is a wire resistor with soldered tappings, and is conveniently wound on a short length of ebonite rod. Its actual value—probably a few ohms—is determined as described below.

*Grid resistor.* This resistor $R_G$ is of special design, and consists of ninety 5-megohm resistors of Karbowid type 2*b* connected in series and embedded entirely in a single block of paraffin wax. For reasons of brevity the method of achieving

this form of assembly cannot be given here, but it should be noted that heatin of the wax to a temperature above that of boiling water is to be avoided. This gri resistor unit is mechanically rigid, and is free from rapid temperature-changes an changes in insulation resistance. These three are all essential requirements. Th unit is undoubtedly more stable than a liquid resistance of the same order. Th number of resistors available when the unit was made up led to the value of 45 megohms rather than the round value 500 megohms.*

*The grid-control unit.* The unit comprising the resistances $R_3$, $R_4$, $R_5$ and $R$ serves to control the grid potentials of the two valves in order to set these at a pre selected value, and to bring the bridge into balance with the photocell obscured. The resistances $R_3$, $R_4$ and $R_6$ are constantan coils of 26 s.w.g. Their values are $R_3$ 25 ohms, $R_6$ 160 ohms, and each of the eight coils $R_4$ 2 ohms. The end points of the 2-ohm coils are brought out each to a pair of sockets. A single wander plug selects the grid potential for valve $B$, no fine adjustment being provided. Vernier adjustment for valve $A$ is obtainable by means of $R_5$. This is a drum type continuously variable potential divider of resistance 4·5 ohms, which may be plugged across any consecutive pair of 2-ohm coils. It will be seen quite simply that if the whole unit is connected to a 2-V. cell as shown, the grid potentials are adjustable between about $-0{\cdot}25$ and $-0{\cdot}4$ V. relative to the negative filament lead, a range sufficient to ensure that the bridge shall be capable of being balanced provided the valves are matched. The reason for the choice of this range is given in § 4.

*The measuring-instrument.* The actual measuring-instrument of the system is a Tinsley general utility potentiometer $T$ of resistance 40 ohms used as a potential divider and fed from a 2-V. cell in series with a plug box $P$. The latter serves to adjust the current in the potentiometer to any desired value, so that the scaling of the instrument can be adjusted to suit the order of photocurrent obtaining in any given measurement. The potentiometer $T$ is of course so connected that the potential applied by its means in the grid circuit of valve $A$ opposes that due to the flow of photocurrent through $R_G$.

*The bridge galvanometer.* The galvanometer $G$ is a Tinsley high-sensitivity reflecting galvanometer of resistance 4620 ohms used with the aperiodic connexion. A scale divided in millimetres is used at a distance of about 1 m. The sensitivity of this galvanometer is approximately 3000 mm. per $\mu$A. at 1 m.

*Batteries.* The batteries employed for the high-tension and photocell supplies are a 140-V. bank of Oldham accumulators of type LHTL of 5·5 ampere-hour capacity. The voltage on the photocell was 20. Two-volt cells of 24 ampere-hour capacity are used for the other supplies.

*Assembly.* On account of the high sensitivity it was thought desirable to adopt a screening system to minimize the effect of stray fields from other sources. The screening system is not, however, complete, the bridge galvanometer and key and the Tinsley potentiometer being unscreened, as a matter of convenience. The valve unit and grid resistor are housed in a metal box, while the photocell is behind these,

* 100 megohm resistors of type 2*b* can now be obtained but their suitability for this circuit has not been tested by the authors.

in a separate compartment, but connected to the valve grid by a wire passing through a small hole in the wall of the compartment. There is also an aperture in the front of this compartment just large enough to admit the light-beam to the photocell. A second separate screening-box holds the three 2-V. accumulators and grid-control unit, the latter being adjusted when necessary through a movable lid. A third metal box contains the high-tension accumulators. Connexions between the systems in the three screening boxes, and those leading to the Tinsley potentiometer are all made with lead-shielded cable, the lead covering being connected, with the boxes themselves, to the negative filament lead. There is no actual earth on the system. The leads to the galvanometer and key, and the key itself, must be well insulated. For this reason unscreened rubber-covered cable suspended in mid-air is used for this short section of wiring, and the key is fixed to a wax slab.

Determination of the values of $R_1$ and $R_2$ is carried out by trial[1,2]. Adjustments are made until the balance of the bridge is not appreciably upset by a small change in voltage of either high-tension or filament battery. An artificial change is suitably obtained by short-circuiting a resistor in the supply lead. After each adjustment of $R_1$ or $R_2$ it is, of course, necessary to re-balance the bridge by means of the grid-control unit, and it is preferable to adjust $R_1$ and $R_2$ alternately, starting with $R_2$, $R_1$ being zero at first.

## § 4. OPERATION

Operation of the valve $A$ under conditions of zero grid current is found to eliminate unsteadiness which is observable when a finite grid current is flowing through the resistor $R_G$, and which is presumably due to fluctuations in this current. The appropriate setting of grid voltage to secure this condition is of course that for which the short-circuiting of $R_G$ (with the photocell disconnected from the valve-grid) produces no effect on the balance of the circuit. It is determined simply by trial, and in the set-up described was found to lie in the neighbourhood of $-0{\cdot}3$ V. relative to the negative filament lead.

The whole electrical system should be switched on and roughly set to balance with the photocell obscured a couple of hours before measurements are to be made. The light source should be fed from batteries and should also be switched on some time previous to measurements. A means of observing the constancy of the operating voltage over the short period taken by one measurement is desirable. Meanwhile the slits of the spectrometer may be set to include the desired wave-length band.* An impurity of about 50 angstroms need not be exceeded with the set-up herein described. For maximum available light at a given spectral purity, the two slits are set at equal widths. To facilitate setting the slits without disturbing other apparatus it is useful to have a mirror or reflecting prism combined with the spectrometer eyepiece, which is inserted between the telescope slit and the lens $L_2$. The eye may then be placed to one side of the optical axis and observations of the slit made at right angles to the latter. The appropriate stray-light filter and the test filter are then placed in their respective holders, and the whole of the exit system

* If measurements are made over a range of wave-lengths, further slit-adjustments may be necessary to maintain the desired spectral purity and at the same time obtain the maximum available light.

is thoroughly screened with black velvet or a black box. The shutter of the spectrometer is then opened and the cell exposed for say 10 min. to the maximum illumination subsequently to be dealt with, the bridge being balanced by means of $T$. This tends to stabilize the effect of any drift in the photocell. Measurements should follow immediately.

The sequence of operations in the measurement of filter transmission is as follows. The desired wave-length is set on the spectrometer and the lamp voltage is maintained steady. With the shutter of the spectrometer closed and the potentiometer $T$ set at zero the valve bridge is balanced to within a few millimetres on the galvanometer scale, by means of the control on the grid of valve $A$. The position of rest of the galvanometer spot with the key depressed is then taken as the null position for the following operations. With the test filter out of the beam, the spectrometer shutter is opened and the bridge rebalanced as rapidly as possible by means of potentiometer $T$ only. The same operations are repeated with the test filter in the beam, and the ratio of the readings of $T$ gives the filter transmission.

The current in $T$ is, of course, set by trial to give a suitable scale reading by inserting resistance in the box $P$. Currents from 0·5 to 8 mA. were found suitable by the authors. It is advisable not to make a change in this current, however, during operation, unless its value has been set to the maximum during the steadying period of 2 hours preliminary to measurements. Further, it is inadvisable to leave the bridge out of balance for longer than necessary, as drift of the bridge is in that case experienced on resetting to balance.

## § 5. TEST FOR LINEARITY

The principle invoked in this test is that of the summation of a number of equal monochromatic light beams. If, say, five beams be projected separately or in combination on to the photocell, the measured effect of the five together should be equal to the sum of the measured effects of the five separate beams, provided that the cell has a linear characteristic. The application of the principle is not easy, however, because it is necessary for each beam to fall on the same area of the cathode and be distributed over that area in the same way. For instance, if a diaphragm with five holes be placed at the lens $L_1$ of figure 1 or between the collimator and telescope lenses, an image of the holes will be produced on the cathode of the cell, and the necessary requirements are not fulfilled. Close conformity to the necessary conditions is secured if the means used to subdivide the light into a number of separately controllable beams is situated in a focal plane of the system, and the photocell cathode and the telescope lens are arranged to be in conjugate planes with respect to the exit lens. Figure 3 shows how this is achieved. The same source $S$ is used and a large condenser $C$ forms an image of the filament about 4 cm. long at $F$. At $F$ is situated a grid of six horizontal blackened narrow steel bars (double knife-edges) which can be clamped in any positions relative to each other. This grid cuts the filament image at $F$ into five parts, and the positions of $F$ and $L_1$ are arranged so that all these five parts are focused by means of $L_1$ upon the collimator slit. Now the five beams of light transmitted by the grid $F$ eventually

reach the photocell as monochromatic beams of almost identical distribution, but of slightly different incidence upon the cathode. They can be allowed so to reach it either individually or in groups by means of suitable shutters at $F$. To render the effects of the five beams equal, the positions of the bars of the grid are adjusted by trial and error. For this purpose it is convenient to bridge them across the poles of a flat U-shaped magnet to which they adhere. When the correct positions have been found, the bars are clamped in place. The shutters may then consist of two pieces of thin tin plate, blackened, which will also adhere in any desired position, and may be moved easily without disturbing the position of the grid. The whole optical system must be as rigid as possible.

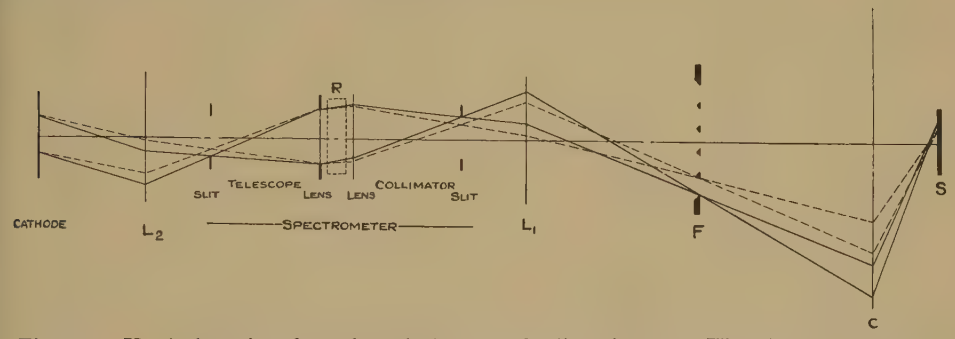


Figure 3. Vertical section through optical system for linearity tests. The dispersive prism is indicated by rectangle $R$. (This diagram is not drawn to scale.)

To test for linearity the spectrometer is set to the desired wave-length, the slits are opened to a suitable value, and the grid $F$ adjusted as described above and then clamped. Measurements are made in the usual way on the five light-beams separately and then on groups up to the five together. It is perhaps useful to repeat measurements on the five beams together several times during the series as a check or to provide a sliding scale of corrections.

For this test the slits should of course be set wide enough to give readings covering the range normally employed in spectrophotometric measurements. There is no objection to this since spectral purity is not of first importance.

Table 1 gives a typical set of results.

The figures in the column headed "ratio" show that the whole set-up is linear at wave-length 4400 A. to a few parts in 10,000. The particular cell and set-up used by the authors gives similar results throughout the visible spectrum, and over the range of photocurrents employed by them.

## § 6. SPECTROPHOTOMETRIC RESULTS

Two separate tables of results, with comparative figures from other sources, are given below in illustration of the performance of the instrument. A signal-green glass affords a fairly stringent test of such an instrument, both as regards working at the blue end of the spectrum where there is not much available energy, and as regards the efficiency of the system of stray-light filters. Accordingly a glass

Table 1. λ, 4400 A. Spectral impurity, approximately 80 A. Lamp voltage, 13·5. Current in potentiometer *T*, 2 mA.

| Beams | Reading | Summary (corrected) | | | |
|---|---|---|---|---|---|
| | | Beams | Reading | Added value | Ratio |
| 1–5 | 12,594 | 1 | 2,183 | — | — |
| 1 | 2,183 | 2 | 2,802 | — | — |
| 1–5 | 12,596 | 3 | 2,628 | — | — |
| 2 | 2,802 | 4 | 2,462 | — | — |
| 1–5 | 12,599 | 5 | 2,519 | — | — |
| 3 | 2,628 | 2–3 | 5,428 | 5,430 | 1·0003 |
| 1–5 | 12,597 | 2–4 | 7,891 | 7,892 | 1·0002 |
| 4 | 2,462 | 1–4 | 10,077 | 10,075 | 0·9998 |
| 1–5 | 12,598 | 1–5 | 12,596 | 12,594 | 0·9999 |
| 5 | 2,519 | | | | |
| 1–5 | 12,599 | | | | |
| 2–3 | 5,429 | | | | |
| 1–5 | 12,598 | | | | |
| 2–4 | 7,892 | | | | |
| 1–5 | 12,597 | | | | |
| 1–4 | 10,077 | | | | |
| 1–5 | 12,596 | | | | |

of this type was used as the subject of the first test, the results of which are shown in table 2.

Table 2. Test filter, B.T. light green 501 N.P.L. 1924. Wave-band, not greater than 50 A. Temperature, 20° C.

| λ in μ | Transmission observed | | | |
|---|---|---|---|---|
| | Photoelectrically 1st set | Photoelectrically 2nd set | Visually (N.P.L. Optics Division) | Percentage difference |
| 0·40 | 0·224 | 0·224 | 0·224 | 0 |
| 0·41 | 0·274 | 0·275 | 0·274 | 0 |
| 0·42 | 0·322 | 0·322 | 0·322 | 0 |
| 0·43 | — | — | 0·365 | — |
| 0·44 | 0·411 | 0·411 | 0·408 | +0·7 |
| 0·46 | 0·480 | 0·479 | 0·476 | +0·8 |
| 0·48 | 0·519 | 0·519 | 0·512 | +1·3 |
| 0·50 | 0·515 | 0·515 | 0·509 | +1·2 |
| 0·52 | 0·448 | 0·448 | 0·449 | −0·2 |
| 0·54 | 0·328 | 0·329 | 0·328 | 0 |
| 0·56 | 0·1993 | 0·1998 | 0·199 | +0·3 |
| 0·58 | 0·1000 | 0·1005 | 0·1011 | −0·9 |
| 0·60 | 0·0444 | 0·0444 | 0·0445 | −0·2 |
| 0·62 | 0·0180 | 0·0179 | 0·0182 | −1·1 |
| 0·64 | $0{\cdot}0068_2$ | $0{\cdot}0068_0$ | 0·0070 | −2·8 |

Calculated integral transmissions for colour temperature 2360° K. are 0·1649 based on photoelectric readings and 0·1649 based on visual readings.

The agreement between the two sets of results is satisfactory, but it was unfortunate that this particular glass, although carefully worked, was not strictly uniform in density over its area. Despite careful centring of the filter in each case, it is probable that the discrepancies shown in the last column of the table are due, at least in part, to this fact.

The second set of results refers to a pale blue glass of the cobalt type which forms one of a set of four photometric filters now distributed among the four national standardizing laboratories. The transmission values of these filters have been the subject of international comparison and agreement[4]. Table 3 gives the mean values obtained at the Bureau of Standards by K. S. Gibson for filter R 3.28, together with the present authors' results obtained more recently on the same filter with the instrument above described. The glasses used as stray-light filters in the latter measurements are indicated also.

Table 3. Spectral transmission of filter R 3.28

| $\lambda$ in $\mu$ | *Gibson's mean values (1929 and 1932) | Preston and Cuckow's photoelectric determinations (1936) | | Stray-light filter |
|---|---|---|---|---|
| | | At 16° C. | At 26° C. | |
| | | Two sets | One set | |
| 0·40 | 0·902 | 0·903 0·902 | 0·902 | Purple |
| 0·41 | 0·896 | 0·897 0·896 | 0·894 | ,, |
| 0·42 | 0·886 | 0·883 0·886 | 0·885 | ,, |
| 0·43 | 0·876 | 0·874 0·876 | 0·874 | Thin cobalt blue |
| 0·44 | 0·866 | 0·867 0·866 | 0·865 | ,, |
| 0·45 | 0·856 | 0·857 0·857 | 0·854 | ,, |
| 0·46 | 0·842 | 0·842 0·843 | 0·841 | ,, |
| 0·47 | 0·816 | 0·816 0·816 | 0·814 | Blue-green |
| 0·48 | 0·778 | 0·780 0·780 | 0·778 | ,, |
| 0·49 | 0·732 | 0·735 0·735 | 0·734 | ,, |
| 0·50 | 0·694 | 0·696 0·696 | 0·695 | ,, |
| 0·51 | 0·643 | 0·644 0·644 | 0·644 | ,, |
| 0·52 | 0·594 | 0·597 0·597 | 0·596 | ,, |
| 0·53 | 0·556 | 0·556 0·556 | 0·557 | Green |
| 0·54 | 0·557 | 0·557 0·556 | 0·557 | ,, |
| 0·55 | 0·596 | 0·596 0·596 | 0·596 | ,, |
| 0·56 | 0·619 | 0·618 0·618 | 0·619 | ,, |
| 0·57 | 0·579 | 0·578 0·578 | 0·579 | Light orange |
| 0·58 | 0·499 | 0·498 0·498 | 0·501 | ,, |
| 0·59 | 0·437 | 0·435 0·435 | 0·437 | ,, |
| 0·60 | 0·441 | 0·441 0·441 | 0·442 | ,, |
| 0·61 | 0·457 | 0·458 0·458 | 0·458 | ,, |
| 0·62 | 0·463 | 0·464 0·464 | 0·465 | ,, |
| 0·63 | 0·459 | 0·458 0·458 | 0·459 | ,, |
| 0·64 | 0·448 | 0·448 0·448 | 0·449 | Selenium red |
| 0·65 | 0·455 | 0·457 0·457 | 0·457 | ,, |
| 0·66 | 0·490 | 0·496 0·495 | 0·494 | ,, |
| 0·67 | 0·561 | 0·567 0·567 | 0·562 | ,, |
| 0·68 | 0·657 | 0·665 0·666 | 0·657 | ,, |
| 0·69 | 0·762 | 0·766 0·766 | 0·759 | ,, |
| 0·70 | 0·835 | 0·840 0·839 | 0·835 | ,, |
| 0·71 | 0·875 | 0·876 0·875 | 0·873 | ,, |
| 0·72 | 0·893 | 0·893 0·892 | 0·890 | ,, |
| 0·73 | 0·900 | 0·899 0·899 | 0·898 | ,, |
| 0·74 | 0·902 | 0·903 0·900 | 0·900 | ,, |

* *P.V. Com. int. Poids Mes.* **16**, 320 (1933).

In the case of the determinations made by the present authors the wave-band used was nowhere greater than 50 A., and in the main it was considerably less. Slit-width corrections are, therefore, in no case large enough to be taken into account. The calculated integral transmissions for colour temperature 2080° K.,

using the international relative-luminosity factors are 0·5239 based on Gibson's mean data, 0·5237 based on the present authors' photoelectric determination at 16° C., 0·5245 based on the present authors' photoelectric determination at 26° C., and 0·5235 for the internationally agreed value[4].*

The differences between the present authors' photoelectric values at 26° C. and Gibson's mean values (which also apply to a temperature of approximately 26° C.) are everywhere small, the greatest deviation being 0·8 per cent at $\lambda = 0{\cdot}66\,\mu$ and the average deviation 0·2 per cent. The present authors' determinations also confirm the temperature effect between $\lambda = 0{\cdot}66\,\mu$ and $\lambda = 0{\cdot}72\,\mu$, as noted by Gibson. Changes in this region do not, however, greatly influence the integral transmission.

## § 7. CONCLUSION

The results so far obtained with the instrument described suggest that it compares favourably with the best visual methods as regards both precision and absolute accuracy. The former is not difficult to attain, in general, with photoelectric instruments; the latter, on the other hand, is usually the main problem. The authors believe it has been solved by employing a type of photocell which possesses almost all of the features desirable from the photometric standpoint, coupled with a stringent method of testing for linearity of the illumination-current characteristic, and close attention to details of design in regard to the electrical circuits and optical train.

## APPENDIX: SOME FIGURES RELATING TO PERFORMANCE

The whole instrument, used under the conditions described above, has been found to be stable as regards short-period fluctuations to within a 1-mm. deflection of the bridge galvanometer. It is also almost free from drift if the compensation of the bridge is carefully carried out. This stability is combined with an effective deflection sensitivity of 500,000 mm. per volt on the grid of valve *A*, a performance which compares favourably with that of a circuit described by Dubridge[3] in which pliotron or electrometer type valves are used. The present authors consider that little advantage is gained by the use of electrometer valves except when a rate-of-charge method is employed, or the very highest sensitivity is aimed at by the use of grid resistors of very high values. The present set-up is not primarily suitable for the rate-of-charge method since the insulation of the valve electrodes is inadequate. The sensitivity mentioned above is equivalent to a galvanometer deflection of 10 mm. for a photocurrent of $5.10^{-14}$ A.

* The internationally agreed value of integral transmission is based on the mean spectrophotometric results obtained by the National Laboratories on the group of four filters, together with comparative measurements of integral transmission. The Bureau of Standards spectrophotometric values are quoted in the present paper as representative of these spectrophotometric results.

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536.626

# MY RECENT PROGRESS IN GAS CALORIMETRY

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*The Nineteenth Guthrie Lecture, delivered May* 4, 1934

I desire in the first place to express to the Physical Society my appreciation of the honour they have done me in inviting me to deliver the Guthrie Lecture. This is an honour which anyone would value, but in my case there is the very special reason for my gratification that I knew Guthrie so well. I owe much to Guthrie. After I had been for a short time at a colliery he brought me back to South Kensington and made me at first his private assistant, and he gave me a life membership of this Society. For the last eight or nine years of his life I was in effect his right hand, and I know more of his ideas and ideals than anyone now living, unless it be his widow whom we welcome here this afternoon. I feel it a duty I owe to this Society to express these ideas, but more especially to the youngest members present. Let me tell you as best I can what Guthrie's feelings were, but in my own words if you will allow a long alliteration. The making of specious scientific surmises unsupported by experiment, however amusing it may be as a pastime or however loudly it may be advertised, does nothing to advance the certain knowledge of the world. The acid test of experiment is essential. So will you who in years to come will have the management of this Society in your hands accept this as a solemn message from the dead. If you would be true to the ideals of Guthrie you will seek for a Guthrie lecturer from among those who have done things rather than from those who have merely talked. *Nullius in verba* is the motto of the Royal Society.

Coming now to the subject of this lecture, the class of calorimeter which alone has been found suitable for examining the calorific value of gas as supplied for public consumption is that known as the water-flow calorimeter, that is, a stream of water is heated by the combustion of a stream of gas. If then the strengths of the two streams are known, and the rise of temperature, the calorific value can be deduced. The water-flow calorimeter was invented by Mr F. W. Hartley of the firm of Messrs Alexander Wright and Co. in the year 1882. When in the year 1905 tests for information on the calorific value of gas had to be instituted I examined three makes of instrument which were developments of Hartley's original calorimeter, but found that in each of them the mercury column in the hot thermometer was jumping about through two or three divisions—tenths of a degree—incessantly, and in consequence these instruments were quite unsuited for tests even for information. I accordingly invented and made the calorimeter known in every gas works in the country affectionately or otherwise as the "Boys", and in this for the first time there was an almost perfectly steady hot-thermometer reading[1]. When in

the year 1920 calorimeter tests superseded illuminating-power tests of gas, the Boys instrument was the best available, and it has carried a heavier burden since then than that for which it was originally designed. I have never considered it all that a calorimeter should be. There are two faults: (1) the acid gases and products of combustion dissolve away the base metal of which the parts are made, and repairs are needed every few years; (2) the instrument is sensitive to unsuitable conditions of use, and special precautions have to be taken to maintain suitable temperatures of the circulating water and surrounding air. Ever since 1920 I have desired to design a calorimeter free from these disadvantages and in other respects as perfect as possible. That used in my recording calorimeter goes only a small way in this direction, and though more particularly in the last ten years I have devoted myself to the design and construction of such an instrument, it was only about a year and a half ago that the unit which I am going to describe later to-day finally emerged, and with this I am satisfied.

Calorimeters are generally used for spot tests, that is, the thermometers are read, gas and water-flows determined, and calorific value deduced, but the greater problem is that of the recording calorimeter in which these operations are performed continuously and automatically and the result through all the hours of the day and night is expressed as a curve. It was this wider problem which mainly occupied me for a few years from the year 1920 and the result, when made without leaking joints and to my design, is quite satisfactory. I am not here, however, to go into any ancient history, but to tell you only about my recent progress in this engaging subject. If you wish to know anything about existing calorimeters you will find a complete and authoritative account in the standard book on the subject, a copy of which should be available in every physical laboratory[2].

You will appreciate that in the spring of last year I must have become fairly saturated with ideas on the subject. I had just completed the construction of the calorimeter unit which I have here this afternoon, and I then intended to graft on to it my water-weighing balance which automatically weighs out the required amount of water every half-minute, and an automatic meter governed by the thinking machine for providing always the same amount of gas corrected for temperature and pressure and water vapour. These elements worked perfectly and I did not seek to improve them or even to simplify them, as that did not seem likely to be possible, and in that frame of mind, without seeking for it, I had a dream on the 4th of May 1933, or saw a vision, call it what you will, but from nowhere there came in the night the unlooked for idea of a tube bent to a right angle rotating about one limb as an axis and carrying a bulb at the end of the other. This as it revolved dipped into water and as it turned the water ran out at the hollow axle. Well, that does not seem much of an invention, for I expect it is as old as the hills, but the interest as I see it now is the impression it made as being of real value. It was evident that it possessed the merit of having no internal working parts. It would go round and round for ever. I was sufficiently impressed by it to get up at six and go to Victoria Street where I blew in glass the bulb and tube you now see. Later, on considering it, the question arose—could the primitive idea be so developed

as to produce something of real value? I do not think I should have developed the theory of what I have running here now if it had not been for the stimulus afforded by that dream, and having developed it and being invited to deliver this lecture I looked anxiously at the calendar to see if the 4th of May should happen to be on a Friday. That settled the date of this lecture.

In any recording calorimeter, provision must be made for dealing with two kinds of variation. The first is that gas in different places is intended to have different values. If the temperature-rise is, as is most convenient, always to be the same if the gas is right, more water should be used with a richer gas. This is a mere matter of gearing, and to the Physical Society I need say no more about this. The real difficulty has always been the varying volume occupied by a gas saturated with water vapour at different temperatures and pressures, for equal volumes as indicated by a meter are not equal quantities. There are two ways of overcoming this difficulty. The first is the one which I adopted of causing the meter-speed to vary in proportion to the volume, which result I effected by means of the thinking machine; the other plan, used by the late Mr J. H. Fairweather, Chief Chemist to the Brentford Gas Company, was to alter the water-flow in the inverse ratio of the volume occupied by gas at any moment. Even though the thinking machine is one of the few elements in the different recording calorimeters at present in use which has never failed or given trouble, and I am by no means prepared to discard its use altogether, the development of the dream invention along these lines affords so elegant an example of applied geometry that it will I trust prove of interest to this Society.

What is required is that the quantity of water used should vary inversely as the gas-volume at the moment. Fortunately the range of variation is not great. In practice a range of 7 per cent above normal volume to 5 per cent below practically meets all requirements, but I have considered the larger range of 10 per cent either way.

In figure 1 I have drawn a hyperbola with a series of equally spaced ordinates, representing volumes from 0·5 to 2·0 by tenths and the corresponding abscissae. The lengths of the ordinates or the levels of the abscissae represent the depths of water in a uniform straight tube which would correspond to the reciprocals of the volumes. I have shaded the regions between $\pm$ 10 per cent, and between $-5$ per cent and $+7$ per cent, and have left the intervening portion of the diagram unshaded, for this is the really important part. I have drawn radial lines from the centre of curvature $P$ of the hyperbola at its vertex to the intersections on the curve, and I have also drawn an arc of the circle of curvature at the vertex. You will notice that these radial lines corresponding to equal changes of volume are not equally spaced. They become progressively more close together as the volumes increase. This is not encouraging from the point of view of instrument-design. As you will see later, however, this progressive change is utilized. It occurred to me that if I used the reciprocal spacing for both sets of co-ordinates and cross-connected them in reversed order as you see in figure 2, I should get a curve which of necessity would be symmetrical about its axis $OP$, as it clearly is. In this the spacing of the radial lines

from $P$ is widest at the sides and is a minimum in the middle, and being a minimum must in that region be very evenly spaced. I did not know what actual curve I should get and I have not found a curve generated in this way described anywhere. Mathematicians have related nearly everything with everything in nearly every kind of way, so that it might be expected that they had done this, as perhaps they have, yet it seems possible that no proper mathematician would deliberately do anything so illogical. Well, I wanted symmetry and I obtained it in this way, and the simple equation above the curve shows that it is a true hyperbola of half the

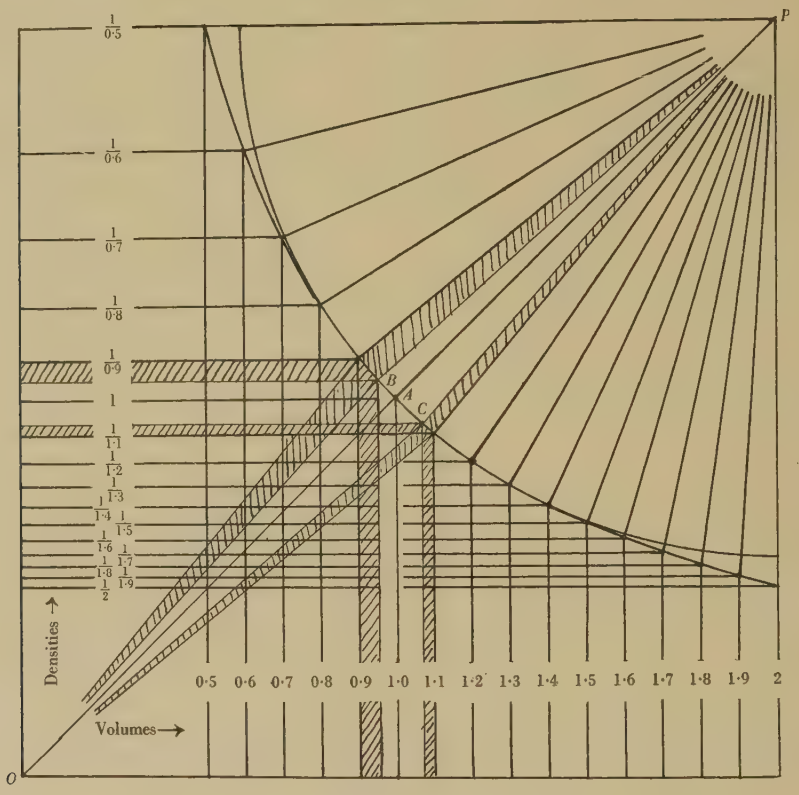


Figure 1.

size with its centre of curvature at $P$. There is an indication of its asymptotes at $Q$ and of the negative branch at $O$. I have drawn also part of its circle of curvature at its vertex.

Now from the point of view of the instrument designer there is here something of real value. Suppose that a pointer centred at $P$, of length $PA$, is made to move through equal angles of the right magnitude by equal changes of gas volume, which is easy; then it will over the required range indicate a series of reciprocal levels with almost incredible accuracy.

The next step from indicating levels to making the water in a trough assume those levels is almost equally easy. A diagram of the mechanism to effect this is

given in figure 3. There is a balance arm $A$ resting on a ball-bearing reel $B$ carrying at one end a long air bell $C$ in mercury and a pointer moving over a scale of gas volumes $D$. The immersion of the tube in the mercury is equivalent to a great stability such as would be produced by a heavy pendulum. This is balanced by the anti-gravity bob or negative pendulum $E$ which can be adjusted exactly; a counter-weight $F$ at the opposite end balances the air tube. When these two balance weights are correctly adjusted the balance arm will remain in neutral equilibrium in any position, even if the stop cock at the top of the air tube $C$ is open. If however it

$$\left(\frac{1}{1+x}-\frac{1}{2}\right)\left(\frac{1}{1-x}-\frac{1}{2}\right)=\frac{2-(1+x)}{2(1+x)}\ \frac{2-(1-x)}{2(1-x)}=\frac{1}{4}\frac{(1-x)(1+x)}{(1+x)(1-x)}=\frac{1}{4}$$

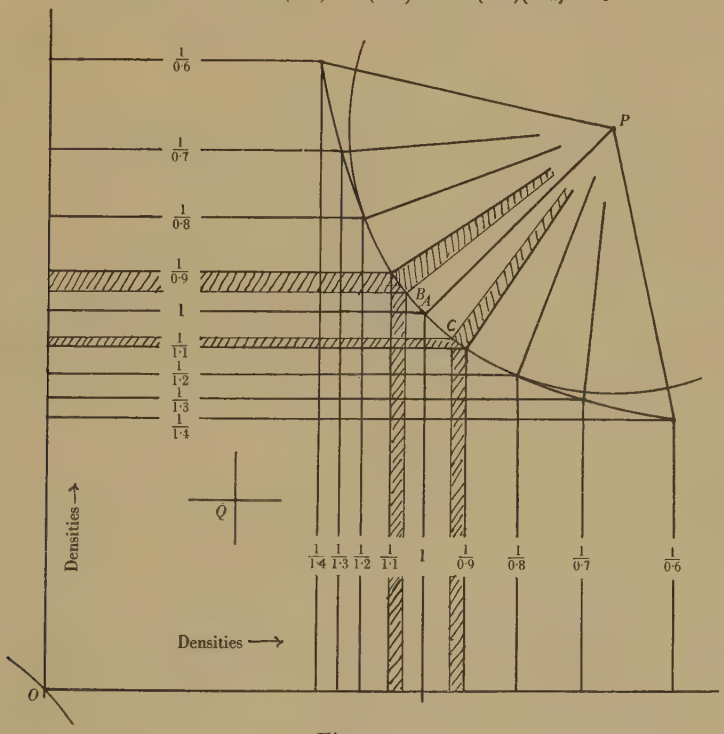


Figure 2.

is closed the bell will change its place as the air within expands or contracts and the actual volume if correctly set will remain so, and it may be read to about 1 part in 1000 on the scale. An identical scale on the air tube indicates the same volumes at the mercury-level, since the ratio

$$\frac{\text{Knife-edge radius}}{\text{pointer radius}} = \frac{\text{area of free surface of mercury}}{\text{area which the mercury would have if the air bell were not in position}};$$

also the pointer radius is equal to half the length of the air column when of standard value. If the volume of saturated gas is required as with a wet meter a few drops of water are let in through the stop cock. If of dry gas the mercury is left dry.

The balance arm carries a siphon $G$, the upturned mouth of which describes a path which lies on the circle of curvature of the half-size hyperbola. The siphon's other leg dips in a front annex of a trough shown dotted, and water is supplied to this trough at a rate somewhat in excess of that at which it will be abstracted by the next part of this mechanism to be described. The water then in the trough will always be at the required reciprocal level.

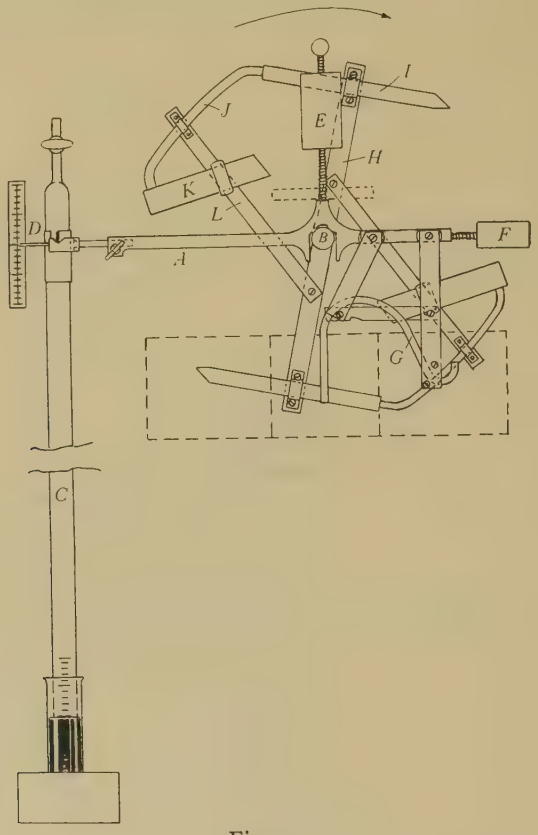


Figure 3.

I should here say that if the mouth of the siphon were left free the water would form a convex dome over it before overflowing, and owing to its surface tension the level of no pressure would be above the dome and the perfection of the geometrical theory would be lost. I have got over this difficulty by springing over the mouth a clip pinching it front and back on a line parallel to the axis. This has the effect of draining water away down to this level, which accordingly is that of the free surface of the water in the trough as required.

It may be interesting to see to what extent the geometrical theory as given approximates to the theoretical requirements. I have accordingly calculated table 1, which needs no explanation except that as I am making 10 in. the unit of my large hyperbola the errors given in inches are errors on 10, not on 1.

Table 1

| Gas-volume | Reciprocals (in.) | Siphon levels (in.) | Error in 10 in. (in.) |
|---|---|---|---|
| 0·90 | 10+1·1111 | 10+1·1012 | −0·0099 |
| 0·92 | 10+0·8696 | 10+0·8644 | −0·0052 |
| 0·94 | 10+0·6383 | 10+0·6361 | −0·0022 |
| 0·96 | 10+0·4167 | 10+0·4160 | −0·0007 |
| 0·98 | 10+0·2041 | 10+0·2040 | −0·0001 |
| 1·00 | 10+0·0000 | 10+0·0000 | 0·0000 |
| 1·02 | 10−0·1961 | 10−0·1960 | −0·0001 |
| 1·04 | 10−0·3846 | 10−0·3840 | −0·0006 |
| 1·06 | 10−0·5660 | 10−0·5639 | −0·0021 |
| 1·08 | 10−0·7407 | 10−0·7356 | −0·0051 |
| 1·10 | 10−0·9091 | 10−0·8991 | −0·0100 |

From this it will be seen that over the useful range the approximation is good to about $\frac{1}{500}$ in. in 10 in. I should here remark that as the long air tube is carried by knife-edges and not by a band over a concentric arc the sine of the angle of tilt is double the gas-volume ratio, it is not the angle itself; and the difference between the sine and the arc reduces the error which would have been caused by support from an arc to about half its value. You must remember that the contact of a hyperbola with its circle of curvature at its vertex is one of the third order and so for a short distance on either side the separation of the two curves is insensible. Or if you prefer to look at it from the analytical standpoint this is given by the known expression $\frac{1}{6}\frac{d\rho}{ds}\frac{(\delta s)^3}{\rho^2}$, the symbols having their usual meanings. The figures in the table followed simply from the use of a table of sines to find the angles of tilt, followed by an evaluation of the consequent motion of the centre line of the siphon mouth.

Having a satisfactory solution of the first step in the problem—that of obtaining reciprocal water levels—I must now pass on to the second step, how to scoop up from the trough quantities of water which are in these proportions and not only scoop it up but raise it considerably so that the water pump, hitherto a necessity, may disappear.

For the purpose of scooping up water I use a water wheel *H* carrying a pair of similar scooping elements. Each of these consists of a main tube *I*, an adjusting-tube *J*, and a pitcher *K* to lift the water well above the axle and there deliver it in a smooth stream without any splash. These are all secured to a spider frame *HL* carried on an axle running in ball bearings. These elements are so constructed that when the water is at the depth corresponding to standard volume of gas the mouth of the main tube emerges from the water when the adjusting-tube is half immersed, and the two contacts with the water subtend a right angle at the axis. The amount of water so picked up is the standard amount required when the gas-volume is 1·000. If the gas-volume is greater the water-level is lower, and when the mouth of the measuring-tube emerges from the water the adjusting-tube is raised above its former position while the water-level is lower to the same extent, and so there is a double

reduction of the quantity scooped up. In addition to this it will be seen that th adjusting-tube, at least in its standard position, cuts the water at an angle of 45 and so the change of length of adjusting-tube containing water is 2 $\sqrt{2}$ times th change of water-level. What then must be the form of curve of the adjusting-tub so that over the whole variation of depth the change of contents shall be in the sam reciprocal proportion as the water-depths? It is here that I am able to utilize th variations in the angles between the radii of figure 1. By the properties of th hyperbola the areas of the vertical strip and of the horizontal strip and of the triangl with its vertex at $O$ and terminating in any arc such as $AB$ or $AC$ are all absolutel equal to one another. These triangles are almost exactly equal to the correspondin triangles with their vertices at $P$, the error here being due to the small curvatur telling in opposite directions in the two cases. The areas of vertical elementar strips represent the water required to be scooped up, so the triangular areas wit their vertices at $P$ are almost exactly in the right proportion and the arcs of th circle of curvature are exactly in the same proportion as these areas. It is onl necessary therefore that the adjusting-tube should be bent to an arc having a radius double that at which its working centre is placed, so that water varying in quantity inversely as the gas-volume may be scooped up. It will be evident that this requires that the balance arm carrying the siphon should turn about an axis absolutely on the same level as that of the water wheel. I make sure of this by using the axle o the water wheel as that on which the ball-bearing reel of the balance arm is mounted.

I have calculated a table of the errors due to the imperfections of the close approximations upon which I have relied in the design. These are not based upon any hyperbolic theory, but starting with the liquid-levels from table 1 and the solutions of many triangles which follow directly from the construction, the actual figures for the calculated water scooped up for the whole range of gas-volumes from 10 per cent below to 10 per cent above standard are given in table 2.

Table 2

| Gas-volume | Arc of hyperbola | Same in minutes | Siphon-level, less 10 in. | Proportional error, 10,000ths of total |
|---|---|---|---|---|
| 0·90 | +12° 40′ 59″ | +760·98 | +1·1012 | −8·3 |
| 0·92 | + 9 56 24 | +596·40 | +0·8644 | −4·9 |
| 0·94 | + 7 18 17 | +438·28 | +0·6361 | −2·6 |
| 0·96 | + 4 46 19 | +286·32 | +0·4160 | −0·9 |
| 0·98 | + 2 20 18 | +140·30 | +0·2040 | 0 |
| 1·00 | 0 0 0 | 0 | 0 | 0 |
| 1·02 | − 2 14 48 | −134·80 | −0·1960 | 0 |
| 1·04 | − 4 24 19 | −264·32 | −0·3840 | +0·8 |
| 1·06 | − 6 28 47 | −388·78 | −0·5639 | +1·8 |
| 1·08 | − 8 28 24 | −508·40 | −0·7356 | +3·0 |
| 1·10 | −10 23 27 | −623·45 | −0·8991 | +5·0 |

Such a degree of perfection in the geometrical basis on which an instrument can be constructed is more than might have been hoped for. Indeed when I was starting on this quest one of a younger generation observed that if I could get

anywhere near it would be a fluke. Now in consequence of my ecclesiastical upbringing I do not like this word fluke but prefer the expression used by a Bishop on a certain occasion. He said, "a merciful dispensation of Providence". I think that is more appropriate, but whether this dispensation was that there should be

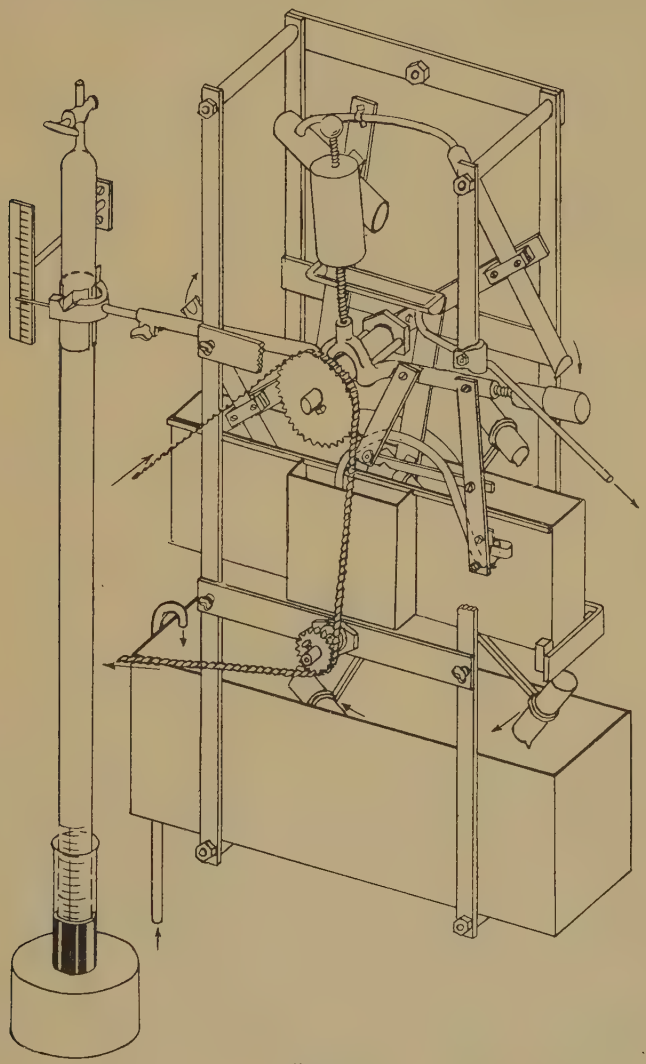

Figure 4.

that half-size hyperbola or that I should have discovered it and all its consequences I do not know. I feel however that I may say in extenuation, "Heaven helps those who help themselves".

It only remains to indicate how water slightly in excess of that scooped up by the water wheel is supplied to the trough in which the water wheel dips. This is very clearly shown in figure 4 which was drawn for me by Prof. C. L. T. Griffith,

using his method of projection which he described at a meeting of the Society o Engineers in November 1932[3].

It will be seen from this that there is a lower main trough which contains wate enough to last for months. A lower shaft running in ball bearings carries fou thimbles and the two shafts are connected by a sprocket chain and sprocket wheel made at almost no cost at all by the Meccano people. This is not the first time have used their units in instrument-construction and I am by no means alone i such use. I should like to say how much may be done to assist experimenta construction by the use of their admirable products. The thimbles in turning ove at the top of their path pour their contents on to an inclined ledge of the uppe trough. In this figure the clip on the siphon mouth is shown in position.

A reference to figure 5 will make the geometry of the position more easy t follow. In this figure $O$ is the axis about which the water wheel turns, $AB$ the

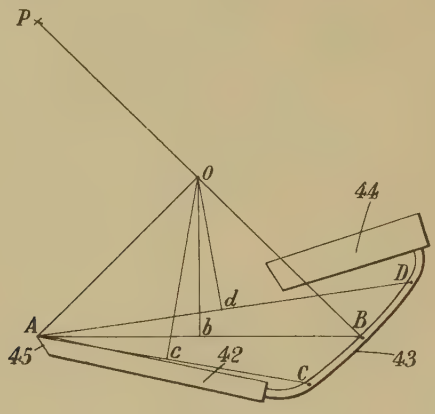


Figure 5.

water-level in the upper trough when the gas-volume is 1·000 and $Ob$ the distance of the water-level below this axis. $AOB$ is a right angle, $PO = OB$, and $P$ is the centre of curvature of the arc $CD$. When the gas-volume is small or the gas dense, $AD$ is an example of the water-level and $Od$ is the distance of the water-level below the axis. Conversely with a low barometer or high temperature, when the gas is expanded and less dense, $AC$ is an example of the water-level and $Oc$ the distance of the water-level below the axis. The distances $Oc$, $Ob$, $Od$ are taken from the previous table and the arcs $BC$, $BD$ are calculated.

Now passing from the mathematical theory of the water-doling unit to practical considerations of design, manufacture and use, I would point out that all the water-measuring parts of the water wheel are made of triblet-drawn brass tubing of which any amount is to be had all identical in bore. These merely have to be cut to length or bent in a bending machine, put together with the spider frame in a jig and there soldered into a metallic whole. Any number can be made exactly alike and the cost need not much exceed so much a pound for brass. Further, in use there is nothing to wear or alter for there are no internal moving parts. After 100 years of continued service there would be no wear or change. The cost of operating this unit is also

so small that it can hardly be expressed. It may require a gallon or two of distilled water a year to make up for evaporation in the closed cupboard in which it operates. This contrasts with one method in use which pours 300,000 gallons of good water into the drains every year at a cost of about £15, a plan which besides being wasteful is liable to be forbidden at any moment in a year such as the present by the Minister of Drought.

The motor which I have used for this purpose is the larger Everett-Edgcombe motor used in synchronous clocks. This is of ample power to turn the water wheel and the auxiliary shaft with its four thimbles and so to do all the pumping that is necessary for the operation of the machine. I have also taken the sprocket chain round the driving-sprocket of the gas pump which forms the second element to be described in this lecture. Thus the one motor does the whole work of the instrument with an expenditure of current which is far too small to show on the ordinary domestic meter.

I have gone more thoroughly into the theory and scheme of the water-doling unit than I shall do in the case of the next two units, the gas pump and the calorimeter unit, because it seemed to me that from the point of view of this society the hyperbolic theory of its operation should have a particular interest.

Before passing on to these, however, I can give you a better idea of the operations of the water-doling unit than you can get from the actual instrument now working, which is too far away from you to be seen, or from the lantern slides of the parts at rest, as I have the advantage of being able to show you through the skill of Fox Photos a kinematograph view of the instrument, first from a distance at which it can all be seen and then by means of a close-up when the silent teeming of the water from the pitcher, the drip from the siphon and the intermittent outflow from the calorimeter can all be seen in turn.

The gas-doling unit which I have to describe next is a complete break-away from all existing practice. The fact is that in no calorimeter yet made, including my own, is the gas as now often supplied to the consumer ever examined at all. In many cases the gas is contaminated with an impurity and it is only the diluted and adulterated gas which is tested, and then some allowance is made to counteract the bad effect of the diluting material. You may not know it, but in the last few years very great changes have been made by the gas-makers in the perfection of their product. I may mention it as a fact, but it is hardly within the scope of this lecture. First the South Metropolitan Gas Company introduced a dry process whereby they were able to reduce the amount of sulphur present in the gas to an extent hitherto considered impracticable, and recently the Gas Light and Coke Company have introduced a wet process with the same object, a most important advance so far as the consumer is concerned, because it is the acid products of combustion that destroy not only calorimeters but geysers and other appliances in which gas is burnt. But that is not the particular improvement which I wish to dwell upon just now. I wish to draw your attention to the effects of a process introduced a few years ago by W. and C. Holmes Ltd., of Huddersfield, who provide plant for drying the gas so thoroughly that it may not even have a dew point at all.

It then has only a hoar-frost point. It is this dry gas that so often the consumer buys, and it is measured almost universally by the consumer's dry meter. All existing calorimeters pass the gas through a wet meter in which it becomes saturated with water vapour and increases in volume in consequence by something over 1 per cent, and it is the calorific value of this diluted gas that under the requirements of Acts of Parliament prior to 1929 has ever been tested in calorimeters. Even where gas is not artificially dried the gas-maker is prejudiced by the pre-1929 methods, because on the whole the gas received by the consumer is not saturated at 60° F. and the difference, small though it be, is in effect a present to the consumer.

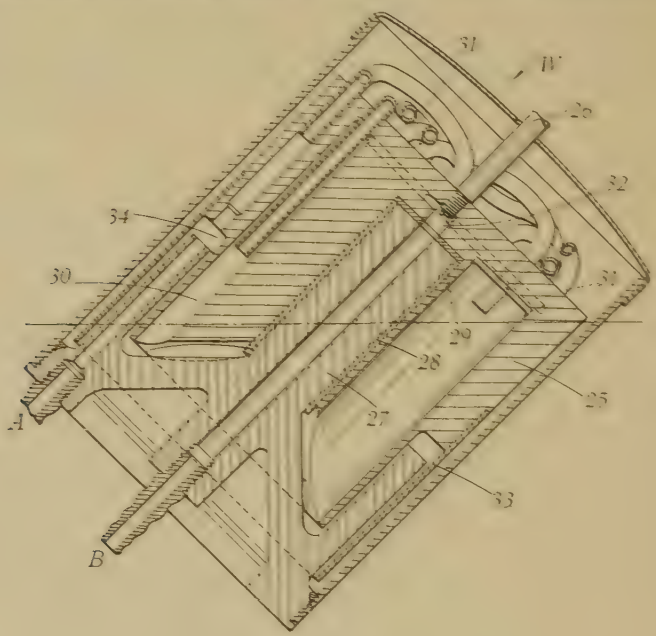


Figure 6.

I had already before the dry gas came into use improved the laboratory wet meter in accuracy out of all knowledge, and I was thus in a position with first-hand experience in meter-design to cope with the new problem. The domestic dry meter has never been made to work with an accuracy such as is essential for our present purpose. After one unsuccessful effort I have succeeded in producing a gas pump driven by external power which so far as I know at present meets all requirements. I designed this after the failure of the first on the 9th February last and it was this lecture looming ahead that made it imperative to find a satisfactory solution. Necessity is the mother of invention most assuredly.

Perhaps the best way to deal with this new invention is simply to show you drawings of the result. Figure 6 is a longitudinal section and figure 7 an end view of the new gas pump and figures 8 and 9 are side and end views on a smaller scale of the levelling-stand which carries it. A cast-iron base 27 carries a cast-iron rotor 25 with a steel tube 28 between for smooth working. The rotor has six

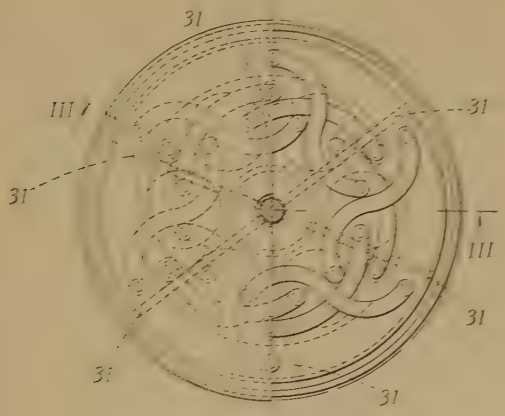


Figure 7.

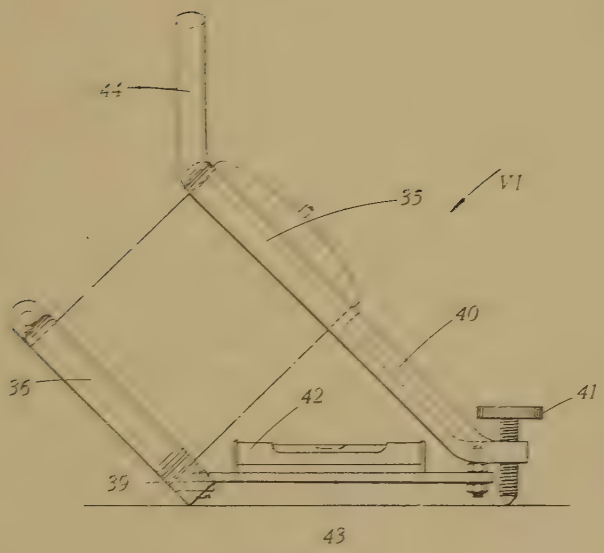


Figure 8.

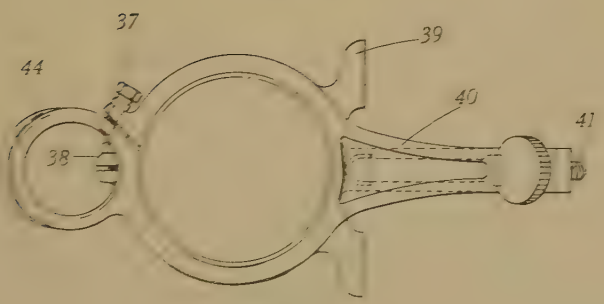


Figure 9.

cylindrical compartments drilled and reamed out truly in all respects, and th interiors of these compartments are connected at the proper times by the steel tub seen at the upper end with inlet and outlet gas spaces by the immersion or emergenc of the twelve tubular passages in mercury. The mercury fills the instrument u to the level line shown cutting diagonally across. The gas enters at *A* and leave at *B*. The rotor is turned by a gear wheel carried on the spindle 26 outside the du cover. Well, that is all. Nothing could be more simple or, as I have found, mor perfect. The delivery of gas resulting from a uniform rate of rotation of the rot is very nearly uniform, having only a small harmonic variation in each 60° o rotation. Successive sixths of a turn are however exactly equal in this respec More important still, the gas burns steadily. There is not a flicker as the severa valve tubes are sealed and unsealed. I mention this particularly because the few people who have gone thoroughly into gas-meter design are aware how difficul it is to avoid such trouble even with water as the fluid. With mercury I expected greater difficulty but I have found none. I made this instrument in three weeks the programme having been—one week-end in the country making patterns; the next week Messrs Sugg and Co. provided excellent iron castings; the second week-end I worked up the castings, putting in a 14-hour day from 9 a.m. to 11 p.m. turning cast iron so as to finish the instrument at once. The third week-end cutting the screws within the steel containing-tube which I had not been able to get before and outside the cast-iron base so as to make a real mercury-tight joint. Then I set it to work and it has run without a hitch ever since. The mercury shows no sign of corrosion or dirt and the turned surfaces on the iron where exposed to the dry gas are only darkened in colour. Should the instrument be used on gas badly fouled with tar fog the rotor can be lifted out at any time and it and the mercury cleaned if necessary, but a proper filter should obviously be interposed in the supply tube. Perhaps I should have mentioned the fact, even though it must be obvious, that the volume of gas doled out at each revolution is $12ar \cot\theta$, where $a$ is the cross-section of any of the cylindrical spaces, $r$ the radius about which their axes are turning and $\theta$ the inclination of the axis of rotation. These quantities are from the nature of the design capable of being reproduced in the workshop with a precision approaching 1 in 10,000, and it is my present view that the capacity of the instrument will be determinable more exactly from its dimensions than from trials with holders, bottles, and such devices. Identity of pressure and temperature in such tests is not too easily attained.

Like the water-doling machine this also I believe to be everlasting and it requires no attention or checking when once it has been set up correctly. I should put in a ball-thrust bearing but that is not shown.

The third element is the calorimeter unit. The general principles on which a water-flow calorimeter depends are so simple that it might be thought that the design of such an instrument should offer no difficulty. When, however, the aim is to make a calorimeter free from the faults of all existing calorimeters and that it should be not expensive, but easy to use, accurate beyond prior instruments, and everlasting, it will be evident that the problem cannot be so very simple. For nine years I was

engaged on this, making, trying, altering and remaking, but I was never satisfied until I produced the instrument I have here now. This I believe to be not only better than any which have been used for testing gas companies' gas, but to be ready to take its place with the most perfect apparatus used in research in determining the heats of combustion of the standard pure gases.

The important features are as follows: (1) Very complete jacketing of all the cold-water parts by incoming cold water, and similarly jacketing of the hot-water parts by the hot water leaving the instrument and jacketing of the section in which the water is rising rapidly in temperature by a shield similarly rising in temperature. While insulation might and no doubt would be used it has practically nothing to do and as now working there is no insulation at all other than quiescent air. (2) Complete elimination of all destructive action by the gases and products of combustion, which may be acid and highly corrosive. This is effected by the use of a pyrex-glass open burner, an ordinary-glass chimney, and an ordinary-glass combustion vessel. While ordinary glass works quite well it might be safer to use pyrex glass for the chimney and combustion vessel. I made the one I am using myself by drawing out an Argand chimney and so making a vessel like a short test tube. These are the parts exposed to hot gases. When the greater part of the heat has been removed the gases and liquid products pass over base-metal cooling surfaces, cast brass and German silver, soldered with soft solder. These are protected on the gas side by a layer of Bakelite varnish specially prepared for me by Mr Sallas of the Bakelite Company. I varnished these parts and baked the varnish on, following his instructions with the most scrupulous accuracy. The result is that though I had this running for some months early last year and then left it to soak for a year and this year have again had it running for some months, the condensation liquid is clear water-white, whereas the corresponding liquid from calorimeters as generally used has a bluish tinge when acid, a bright blue colour with ammonia, and when it is treated with ferrocyanide of potash there is a deep chocolate colour. Not only is the condensation liquid when acid not coloured but whether treated with ammonia or ferrocyanide it is still clear water-white when seen through a depth of 15 in. It is for this reason that I put forward the suggestion that the instrument is everlasting. What I cannot tell of course is whether the acid attack will in time damage the bakelite. When cooled the gases and condensed products pass through a tray and tube of ebonite which also resists their action.

Remaining features are of course complete counter-current interchange of heat between the water and the gases and careful proportioning of gas and air for combustion and means for determining any very small want of balance in the amount of sensible heat entering and escaping with the gases, and the highly important effect in the case of dryish air and gas of more water vapour leaving the instrument than enters. This is important on account of its great latent heat, and this in spite of the fact that with gas as supplied burning in air there is a contraction of about $1\frac{1}{3}$ times the volume of the gas. Major Hyde has given me the figure of 5° F. as being approximately the proper excess over inlet-air temperature for there to be a perfect balance in this respect. When we come to determinations of calorific value

much exceeding 1 in 1000 in accuracy, then the question of definition becomes important, but that I have on the present occasion no intention of discussing.

With these general observations there is little more to say than to refer to figure 10, which is a vertical section of the instrument, while figure 11 is a vertical

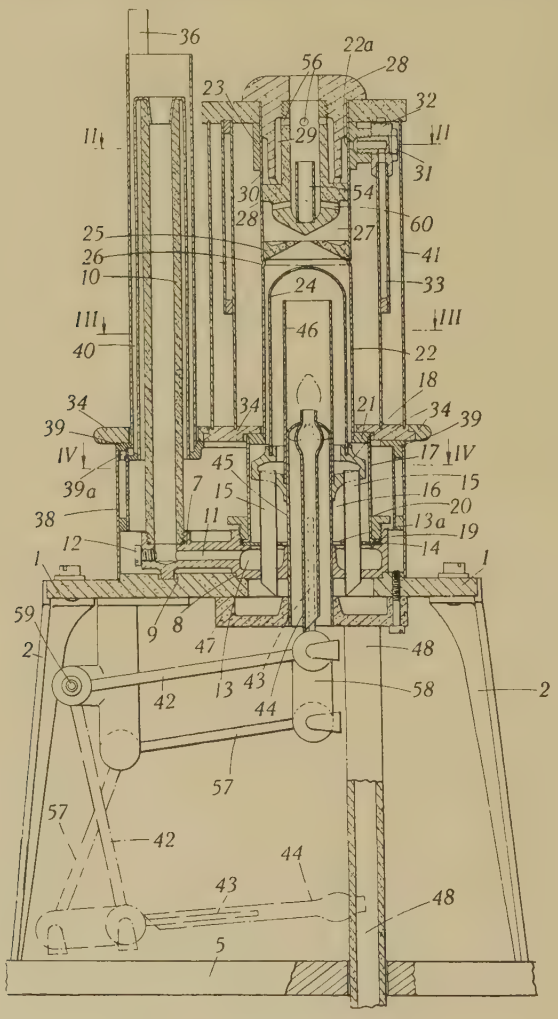


Figure 10.

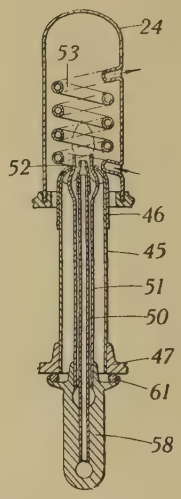


Figure 11.

section of an alternative element specially designed for work of the highest accuracy on the heats of combustion of the standard gases.

The cold water enters at 36, circulates through the cold-water jackets 38 and 40, and passes through the cold-water thermometer pocket 10. It then passes twice past the German silver cooling tubes 15, up round the combustion vessel 24 and the hot thermometer pocket 54, then through the hot-water jacket 33, and out by a tube at the back which cannot be seen in the figure. The gas is brought in to the

parallel motion burner support at the swivel 59. The pyrex-glass burner tube 44 is pushed up against claws to locate it, so that the air-gas flame is central and always in the same position. 46 and 24 are the chimney and combustion vessel each made of ordinary or pyrex glass. The hot gases descend through the glass-walled annular space, then through the tubes 15, and out through the dish 47 and down tube 48. This latter is provided with exit thimbles of various sizes so that the flow of gases may be restricted to that which gives the greatest rise of temperature. Actually there is no observable difference for a small variation until the air becomes insufficient, when the reduced rise is manifest and the smell of the exit gases becomes pronounced. In the laboratory the chemical analysis of the products of combustion gives all desired information as to air-gas ratio, but the thimble plan is simpler and is more adapted to routine operations. Provision is made for ascertaining the temperature of the exit gases at the top of the tube 48 and of the inlet air alongside, which is also that of the gas.

With the calorimeter as I have made it the exit gases are only $1\frac{1}{2}°$ C. above the inlet water. An increase in the lengths of the tubes 15 would reduce this very slight loss of heat, but it is better to allow for this rather than eliminate it by undue lengthening of the cooling tubes.

It may be interesting to observe that the hot thermometer settles down to its steady value in 15 min. from that of starting up all cold, and that though the water supply is intermittent with a half-minute period the hot-water thermometer shows no fluctuation of as much as $\frac{1}{100}°$ C. even with a temperature-rise of 20° C. These are both valuable results which I have not seen approached in any other water-flow instrument. The hot exit water passes on its way to the cooling coils below, from which it returns by a tube shown to the left of the lower tank in figure 4.

If the temperatures are accurately determined and the deliveries of the water and gas are verified the limits of accuracy are settled by the limitations of the thermometers for measuring the temperature-rise of the water, whether these are mercurial or electric, and this limitation applies to all calorimetry. For the most accurate determination of the heat of combustion of standard gases it would be advantageous not to have so great a proportion of nitrogen in the exit gases, for this adds to the difficulty of complete cooling in the heat-interchanger as well as to the determination of the heat balance between incoming and outgoing gases. For this reason I have provided for this special purpose the variation in design of the burner and combustion chamber shown in figure 11. In this 58 is the link of the parallel motion, but the gas to be burned is brought in by the lower swivel pipe 57 and oxygen or other gas according to the reaction to be examined is brought in by the upper swivel pipe 42. The burner tip 52 may be made of fused silica and the oxygen or other gas tube 51 of pyrex glass or silica. The combustion vessel 24 is now provided with a water-cooled worm, so that the gases from the intensely hot flame may be cooled in passing up and so strike the top of the dome without harm. This part might be made of transparent fused silica for safety, but I have found that pyrex glass is not damaged and it is very pretty to see the formation of steam in the worm. This being lighter than water hurries the circulation. The top

of the link 58 carries a closing dish with an umbrella ring 61 to make a sufficiently gas-tight joint.

It will be evident from an inspection of the drawing, figure 10, that the calorimeter unit has been designed to take a mercurial thermometer, or if required the "bulbs" of a differential electrical thermometer which need occupy but little more space than is required by a mercury thermometer. For this reason I am unable to use the large vessels containing amyl alcohol which actuate the recording pen of my present recording calorimeter. While these work perfectly when the soldered joints do not leak they have two disadvantages which I wish to avoid. The first is the great heat capacity. The second and more serious disadvantage is the extended surface from which transfer of heat to the outer world may take place, or the increased difficulty of insulation. I have therefore decided to adopt the differential electrical thermometer with recording mechanism. Several of these already exist and I have not in consequence undertaken the design and construction of a new one. I have ideas on this subject but it seems best in the first instance to use what is already available.

In this lecture I have omitted all description of details of necessary adjuncts which, being more or less commonplace and obviously essential to the use of the instrument, would detract from the interest which I hope will be found in the originality of the three units which I have described.

Anyone who would accept the honour of being the Guthrie lecturer would wish that he had something new and valuable to describe, but in my case seeing how much I owe to the late Prof. Guthrie, and to the terms in which at the beginning of this lecture I have interpreted the soul of Guthrie, more than ever is it essential that I should have something which with my recollection of him I know would have met with his approval; I hope I have succeeded.

*Note.* Owing to the official character of my work in designing these units with a view to their being adopted by the Gas Referees, of whom I am one, for testing the gas supplied to the public it has been necessary to make a formal agreement with a manufacturer. Official patents had been obtained by a Secretary of the Board of Trade and myself as inventor and a formal licence had to be granted to a manufacturer. Messrs Casella and Company Limited were selected. I have kept back the publication of this lecture until I could give the story complete up to this stage, and now (February 1936) I have received the agreement signed by a Secretary of the Board of Trade, and Messrs Casella are able to start on the making of the first unit.

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# THE CONSTANCY OF THE VELOCITY OF SOUND AT SONIC FREQUENCIES

By F. A. WALCH, Ph.D.

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*ABSTRACT.* A method of investigating whether there is any dispersion of sound waves in air at sonic frequency is described. It consists in comparing the wave-form of a complex sound at different distances from the source. A technique for making the necessary collodion diaphragms has been worked out, and a simple but effective drum camera constructed. A controllable wave-form is provided by two oscillators, one tuned to a fundamental and the other to a harmonic, locked into synchronism by close coupling. Frequencies from 250 to 1000 c./sec. have been investigated. Velocities have been proved constant for the lower frequencies to within 1 in 500 and for the higher frequencies to within 1 in 1000.

## § 1. INTRODUCTION

The constancy of the velocity of sound with frequency at audio frequencies always seems to be taken as an accepted fact. It appears, however, that no accurate and systematic investigation over the whole audio range has ever been undertaken by any one observer. Diverse methods in the open air and many varieties of tube methods give results which agree fairly well with one another, but of the direct methods those giving consistent results have been undertaken either at one particular frequency or over a very limited range of frequencies. It has never been directly proved that the velocity of sound, say between frequencies of 200 and 1200 c./sec., does not vary by more than 1 in 1000. Various qualitative arguments are advanced in support of the constancy of the velocity. Of these the most common is that when an observer listens to an orchestra playing at a distance he hears the piece performed in correct time just as it is being played. If sound of different frequencies travelled at different rates it is obvious that the time of the piece would be disturbed, chords would be broken into their constituent notes, and in an extreme case even the sequence of the notes might be altered.

On considering this argument quantitatively, it is found that such constancy as it establishes is useless as far as scientific measurement is concerned. It is well known that there is a phenomenon, persistence of hearing, analogous to persistence of vision. Thus, two sounds separated by a time interval of less than $\frac{1}{10}$ sec. are not heard as two separate sounds. Applying this to the above argument, we see that if we are to be able to detect a disturbance in the music due to sounds of different frequencies travelling with different velocities, then one note must gain upon the other to such an extent that they arrive at the ear of the observer with a time displacement from the original of more than $\frac{1}{10}$ sec. If the velocity of sound is taken to be 300 m./sec., it is easily shown that in order to detect a change even

as large as 1 per cent, i.e. 3·3 m./sec., one must be about 3000 m., i.e. 2 miles, from the source. At this distance not even the loudest of brass bands would be heard with sufficient distinctness to enable one to make observations.

The experiments to be described were undertaken with the object of investigating the constancy of the velocity of sound by a direct method at frequencies covering the audio range. The phenomenon described above was applied quantitatively. The wave-forms of complex sounds, i.e. of sounds containing a number of different frequencies, were photographically recorded at different distances from the source and analysed. If one harmonic of the complex wave has a velocity differing from that of the others, then there will be a change of wave-form. The change in phase-difference between the two harmonics with distance gives a measure of the velocity-difference. The method, therefore, only measures velocity-differences but small differences cause quite large phase-changes and the method is independent of such factors as temperature and humidity, since it in no way depends on the actual value of the velocity.

Another great advantage is that the fidelity of the recording apparatus, i.e. the agreement between the actual air-pressure variations received by the recorder and the wave-form recorded on the photographic film, does not need to be of the high order that is required, for instance, in speech-recording of the type undertaken by Crandall and Sacia[1], whose experimental efforts were mainly directed towards the attainment of this high fidelity. In other words, resonance effects of various kinds, in loud-speaker, horn and diaphragm, which are always present in all types of recording instruments and are usually one of the main sources of difficulty, are not, in this case, of major importance. Only differences between very similar wave-forms are required and not the wave-forms themselves. The effect of resonances in the present work was to reduce the accuracy with which the wave-form of a trace could be predicted from an examination of the electrical output of the oscillator with the cathode-ray oscillograph.

Since high fidelity was not essential it was possible to use an apparatus which is simple compared with the calibrated condenser microphone and high-fidelity multi-stage amplifier usually necessary for sound-recording.

The detecting apparatus consisted of a thin collodion membrane on which is deposited a gold, silver or platinum mirror so thin that its mass does not affect the vibration of the membrane. By means of a beam of light reflected from this mirror a photographic trace is obtained showing its motion. Before the apparatus is described in detail, however, it will be interesting to get some idea of the value of the phase-variation which it is hoped to measure.

A sound-wave falling upon the membrane can be represented by

$$y = A \cos \omega (t - x/c),$$

$x$ being the distance from the point where the displacement is $y = A \cos \omega t$. For simplicity consider a complex wave consisting of a fundamental and one harmonic only:

$$y_0 = A_1 \cos (\omega t + \epsilon_1) + A_n \cos (n\omega t + \epsilon_n),$$

where $y_0$ is the displacement at $x=0$. Now suppose the distance from source to membrane be increased by an amount $x$. Then we have

$$y_x = A_1' \cos\left(\omega t + \epsilon_1 - \frac{\omega x}{c_1}\right) + A_n' \cos\left(n\omega t + \epsilon_n - \frac{n\omega x}{c_n}\right),$$

where $c_1$ and $c_n$ are the velocities of the 1st and $n$th harmonics of frequency $\omega/2\pi$ and $n\omega/2\pi$ respectively. Then the phase-change between the 1st and $n$th harmonics due to the difference between their velocities is obtained from

$$y_0 = A_1 \cos(\omega t + \epsilon_1) + A_n \cos(n\omega t + \epsilon_n),$$

$$y_x = A_1' \cos\left(\omega t + \epsilon_1 - \frac{\omega x}{c_1}\right) + A_n' \cos\left(n\omega t + \epsilon_n - \frac{n\omega x}{c_n}\right),$$

whereas if the $n$th harmonic had the same phase-velocity as the fundamental we should have

$$y_x' = A_1' \cos\left(\omega t + \epsilon_1 - \frac{\omega x}{c_1}\right) + A_n' \cos\left(n\omega t + \epsilon_n - \frac{n\omega x}{c_n}\right),$$

$$\text{Phase-change} = n\omega x\left(\frac{1}{c_1} - \frac{1}{c_n}\right) \text{ radians},$$

$$= 2\pi n f x\left(\frac{1}{c_1} - \frac{1}{c_n}\right) \text{ radians},$$

since $\omega = 2\pi f$, where $f$ is the frequency of the fundamental

$$= 360 n f x\left(\frac{1}{c_1} - \frac{1}{c_n}\right) \text{ degrees},$$

and is positive if $c_n > c_1$.

Suppose we wish to detect a velocity-difference of 1 in 1000, so that

$$c_n = \frac{1001 c_1}{1000},$$

$$\text{Phase-change} = 360 n f x\left(\frac{1}{c_1} - \frac{1000}{1001 c_1}\right)$$

$$= \frac{360 n f x^\circ}{1001 c_1},$$

$$= 0{\cdot}36\,\frac{n f x^\circ}{c_1} \text{ approximately.}$$

As the phase-changes are themselves small, the approximation gives negligible error.

As an example consider a typical wave-form made up, say, of frequency 400 and its 3rd harmonic of frequency 1200, the distance $x$ being 300 cm. Substituting in the above formula we get

$$\text{Phase-change} = \frac{0{\cdot}36 \times 3 \times 400 \times 300}{33{,}000},$$

$$= 4^\circ \text{ approximately.}$$

Thus, under these conditions, if we can measure phase-changes to within 4°, we can detect velocity-changes of 1 in 1000 between the frequencies 400 and 1200. The accuracy with which the phase-changes can be measured will be determined by (1) the accuracy with which measurements can be made on the photographs, and (2) the greatest distance $x$ for which measurements are possible. These two factors are interconnected, in that if $x$ is to be large, then the magnification of the system must be made large and consequently definition suffers. The way in which these two factors have been combined to give the most favourable conditions will be discussed in the description of the apparatus.

## § 2. THE APPARATUS

The detecting apparatus as first set up is shown diagrammatically in figure 1. It consisted essentially of a box $B$ specially designed to carry the sound-sensitive collodion membrane $D$, upon which was deposited a gold mirror. Light from a

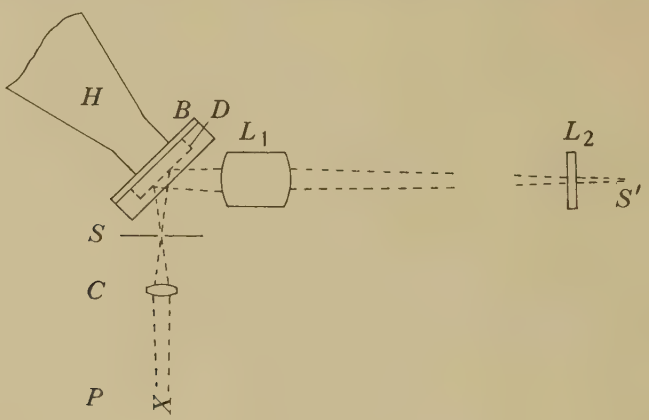



Figure 1.

Pointolite source $P$ passed through a condensing-lens $C$ and a slit $S$ placed close to it, $C$ forming an image of the Pointolite bead on the membrane mirror, which reflected the light through 90°. A projection lens $L_1$ formed an image of the slit $S$ on the photographic film. A cylindrical lens $L_2$ condensed the image to a fine tracing point at $S'$. Use was made of a spool-and-sprocket camera of standard design, capable of a maximum film-speed of about 3 m./sec.

Traces obtained with this preliminary apparatus showed that the optical definition and brightness of the tracing point were insufficient for accurate analysis. The wave-length of the traces was too short, so that a higher film-speed was necessary. An increase in the acoustical sensitivity of the apparatus also was needed to lessen the volume of sound necessary to produce a sufficient amplitude. This would reduce the possibility of interaction between source and receiver, which was the probable cause of some of the large changes in wave-form with distance obtained with the preliminary apparatus.

## § 3. THE OPTICAL SYSTEM

The original optical system was considerably modified. This system, which is very popular in talking-picture sound-recording[2], gives good definition, but the amount of light concentrated into the spot is small since only a tiny fraction of the total light emitted by the source is used; the ratio of the actual area of the slit to the area of the whole sphere receiving light is extremely small.

The system eventually adopted is shown in figure 2. Light from the arc $S$ was concentrated by means of a condensing system $C$ upon a fine wire $O$, 0·002 in. in diameter, which formed the object. $S$ was not focused upon $O$, for this gave rise to unequal illumination of the final image, and the divergence of the rays after leaving $O$ rendered difficult the formation of a small intense spot on the diaphragm mirror $M$. Actually the arc crater was focused on to the mirror, or, what was practically the same thing, on to the diaphragm $D$ which was used to exclude the extreme

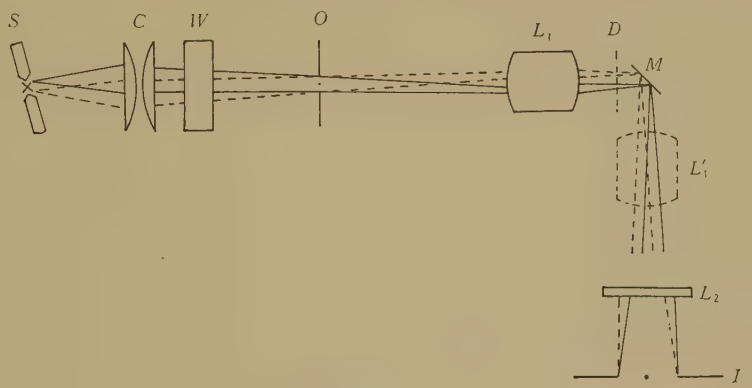



Figure 2.

rays. This was easily visible, and was therefore used for focusing-adjustments in preference to $M$, which was not easily visible. A water cell $W$ was included between $C$ and $O$ to prevent undue heating of the membrane at $M$. The image of $O$ was projected in the earlier experiments by a 20-cm. Leitz projection lens at $L_1'$. It was later found advantageous to have the projection lens at $L_1$. The image of the fine wire $O$ was formed at $I$ and condensed down on to the camera slit by the cylindrical lens $L_2$.

The use of a fine wire as object was dictated by the limitations imposed by the use of a slit. The fine wire has the fundamental advantage that the effect of diffraction is to narrow instead of spread the image. It is also easier to obtain a small spot on the mirror $M$ if a fine wire is used as object. Platinum wires of diameters 0·0005–0·004 in. were tried; a diameter of 0·002 in. was found to be most satisfactory. This was stretched as tightly as possible across a washer of internal diameter $\frac{1}{4}$ in. and the ends were fixed with two small dabs of sealing wax.

By shifting the projection lens from $L_1'$ to $L_1$, thereby using the whole optical arm $MI$, the displacement of the image at $I$ for an angular deflection $\theta$ of the

mirror $M$ was increased from $150\theta$ to $360\theta$, while the actual size of the image was only increased in the ratio $2{\cdot}7 : 3{\cdot}5$. To keep the latter ratio as small as possible $L_1$ was placed as close as possible to $M$.

It is essential for the formation of a well-defined spot that the object wire should be straight and vertical. Deviations from either of these conditions are accentuated when the light is focused down with the cylindrical lens.

## § 4. THE PREPARATION OF COLLODION MEMBRANES

It was realized at an early stage that with the original apparatus the intensity of sound necessary to give a reasonable amplitude was much too great. Apart from the general unpleasantness of working with such high intensities, it was highly probable that interaction was taking place between the source and the receiver. One way in which it was possible to increase the sensitivity of the apparatus was to employ a larger and less tightly stretched receiving-diaphragm. The technique developed for the preparation of these films will now be described.

The method employed for preparing flexible collodion films followed closely that of Dewhurst[3]. Since the films had to be sputtered, a number of breakages was to be expected. It was therefore essential to be able to make a new film without much preliminary preparation.

The solution used was prepared from the standard solution sold by Griffin and Tatlock, which consists of 3 g. of guncotton dissolved in 100 cm.$^3$ of 1 : 1 mixture of ether and methylated spirit. 3 parts of ether were added to 2 parts of the above solution and to this was added 1 part of a 2 per cent solution of castor oil in ether. This solution gave films showing red and green interference colours by reflection, clear and brilliant, and of sufficient strength to stand sputtering without wrinkling or going slack.

The mercury on which the films are to be spread must be very clean and the surface must be free from dust. It is twice passed through dilute nitric acid, twice through distilled water, and is then distilled *in vacuo*. Difficulties due to particles of dust, etc. falling on to the surface are greatly lessened by the use of a dust-free room which exists in the department, designed and furnished specially to reduce the amount of dust in the air to a minimum. The mercury is contained in a shallow dissecting-dish about 15 cm. in diameter and about 1·5 cm. deep. This can be covered by a similar dish of slightly larger diameter to protect the film while this is being dried. Several preliminary films are now formed over the whole surface, allowed to dry, and stripped off. This is necessary so that the collodion may wet the surface; wetting is shown by rapid even spreading in approximately circular shape. When this condition obtains, a few cm.$^3$ of the prepared solution are poured on to the mercury in sufficient quantity to form a film of the required diameter. The solution spreads rapidly at first, then more slowly, and finally starts to contract. The rate of contraction increases until wrinkles begin to appear at the edges of the film, It was found that this was the best time to remove it. The steel ring, turned from Brown and Sharpe's Ground Stock $\frac{3}{16}$ in. thick, $2\frac{1}{2}$ in. in internal and $3\frac{1}{2}$ in.

in external diameter, on which the film is to be carried is prepared by coating it thinly by means of a small brush with a dilute solution (5 to 10 per cent) of Canada balsam in ether. The ring is made to adhere by placing it on the film just before it dries. The excess film round the edges of the ring is removed with a fine needle. The removal of the film from the mercury surface is rather a tricky operation. If, however, the surface is really clean, then, by careful manipulation and a combination of a lifting and sliding motion, films up to $3\frac{1}{2}$ in. in diameter can be removed straight away from the mercury surface. When the film is removed it is found to be loosely stretched, but tightens up as the underside of the film dries.

A milky appearance due to dampness can be removed by gentle heating. The heating must not be too prolonged or too intense since it has a tightening effect. A film so tightened, if left for several hours, becomes much slacker than it was before being heated. The films have to stand considerable heating during the process of sputtering. It is, therefore, advisable to keep them as taut as possible.

The use of ground stock for the rings ensured that they were accurately flat. The size chosen is the largest convenient size for which the percentage of breakages is small. Moreover, sizes larger than this may be comparable with the wave-length of some of the higher harmonics, in which case the pressure due to the incident sound-wave may not be uniform over the film.

## § 5. SPUTTERING ON TO COLLODION FILMS

The apparatus and method of sputtering were similar to those used by Andrade and Martindale. For further information reference should be made to their paper[(4)].

The apparatus was as described by them, except that the small central disc was replaced by a gold-plated diaphragm, of diameter 3 in., fastened to the cathode by small aluminium clips. The uniformity of deposition necessary to obtain a good mirror did not require a guard-ring.

The Cenco Hyvac pump is connected direct to the jar through wide-bore tubing. A side branch contains a needle-valve leak to let air in slowly when required so as not to burst the collodion film with a sudden rush of air. The absence of traps and the like enormously increases the rate of evacuation, and any oil vapour which may diffuse over does not seem to have any appreciable effect on the rate of sputtering.

The anode is covered by a glass disc upon which is placed the film to be sputtered. The interposition of the disc insulated the film from the anode and there was no tendency for the discharge to pierce the film in order to take the shortest route to the anode. The glass disc does not materially affect the rate of deposition. The process is in many ways analogous to one of evaporation from the cathode and condensation on the cooler parts of the apparatus. This is well shown by the deposit on the sides of the jar, the base plate, and, to a smaller extent, on the back of the cathode.

The circuit used consists of an induction coil working off the 200-volt mains, the primary current being interrupted by means of a mercury break with two large

condensers in parallel. The secondary is connected to the sputterer, and the current measured by a milliammeter of rectifier type. The voltage is estimated by means of another a.-c. milliammeter and a 1·5-megohm resistance, the two being connected in parallel across the sputterer electrodes.

The conditions under which sputtering was carried out were approximately as follows: electrode-distance, 3 cm.; pressure, 0·01 mm.; r.m.s. current, 25 mA.; r.m.s. voltage, 1000 V.; dark space, 1·5–2 cm. In general, on switching on, the voltage drops rapidly at first to a constant value, remains steady for some time and then, if the apparatus is working properly, rises steadily to a constant maximum, the current meanwhile decreasing to a minimum.

Attempts were made at first to sputter a small gold mirror about 0·7 cm. in diameter on the diaphragm. The film was covered by a mica mask containing a hole of the required diameter. On exposing the film to the discharge for a sufficient time to obtain a good mirror, however, the film round the edges of the sputtered area developed a series of small wrinkles radiating from the mirror. It was not found possible to remove these wrinkles, which were probably due to heat radiation from the hot cathode. After 15 minutes' sputtering this was too hot to touch.

To overcome these wrinkling effects the films were sputtered all over so that the final tension would be uniform. The films were made as taut as possible to begin with. The actual sputtering was carried out intermittently, intervals being allowed for the apparatus to cool down. Periods of 10 min. for sputtering and cooling were found to be satisfactory, and the total time to form a mirror was then about 2 hours. The tension of the films was reduced by sputtering, but they still remained sufficiently taut to be used as membranes.

During these experiments it was observed how similar this process is to evaporation and condensation and therefore what an important part the temperature of the electrodes plays. This temperature factor seems to be neglected entirely in the various empirical formulae put forward to give the rate of deposition in terms of discharge constants—current, voltage, pressure, length of dark space. Most experimenters fix the anode temperature by water cooling, but the temperature of the cathode and its effect on the rate of sputtering has been entirely neglected. It was observed in the present experiments that variations in current and voltage had a marked effect on cathode-temperature. It seems likely that some of the complicated effects of variation of current and voltage on the rate of sputtering may be explained in terms of cathode temperature.

## § 6. EXPONENTIAL HORN

To fix these membranes in the apparatus the original diaphragm-holder was removed and the steel ring held in two small brass clamps. These also held a tube of the same diameter as the membrane for connexion to the horn.

The horn was exponential, of length 44 cm., its mouth being 48 cm. in diameter. By measuring the diameter of the horn at various distances from the throat a value

for $m$ was obtained in the formula $S = S_0 e^{mx}$. Actually $m = 0{\cdot}12$, giving a cut-off frequency of approximately 300 c./sec. and horn resonances appreciably below 430 c./sec. As has been explained previously, resonance effects are not of fundamental importance. Experimentally it was found that response did not fall off appreciably till a frequency of 220 was reached, probably due to the increased diaphragm response in this region (diaphragm resonance 80 c./sec.).

The speaker employed was an experimental type kindly lent by Electrical and Musical Industries. It consisted of a large, thin, stretched aluminium diaphragm of diameter 38 cm. rigidly clamped between two steel rings. A small speech coil was eccentrically mounted on the diaphragm and moved between the poles of a mains energized electromagnet. The speaker was capable of handling an enormous range of volume without appreciable amplitude distortion.

## § 7. THE CONSTRUCTION OF A HIGH-SPEED DRUM CAMERA

To make possible a higher film-speed than the 3 m./sec. obtainable with the original spool-and-sprocket camera a drum camera was built to hold two $4\frac{1}{2}$-ft. lengths of film and capable of a maximum film-speed of 50 ft./sec. Actual photographs were taken at speeds up to 32 ft./sec. The traces for analysis were obtained at lower speeds owing to the improved definition thereby obtained.

The camera, which fulfilled many of the purposes of an expensive high-speed camera, was of an extremely simple design due to Prof. Andrade. The total cost in all was not more than £5.

The drum consisted of a standard belt pulley supplied by the Skefco Ball Bearing Co., and had a diameter of 18 in. and a rim-width of $4\frac{1}{2}$ in. It was mounted on a shaft supported by standards and self-aligning ball bearings supplied by the same company. These were mounted upon a bench which could be wheeled into the dark room for loading and unloading.

A light-tight box surrounded the drum and standards. The front of the box carried two slits $\frac{1}{10}$ mm. wide and 4 cm. long, made from razor blades, the outside ends of the slits being opposite the outside edges of the drum. For loading and unloading there was a door 6 in. wide at the back of the box, fastening tightly down on a seating of velvet.

The drum is driven by a small electric motor carrying a 4-in. pulley driving on to a 12-in. pulley on the drum shaft. With this arrangement the motor is self-starting and attains a speed of 900 r.p.m. in about 2 min.

The fixing of the film to the drum presented some difficulty. Various methods were tried, but were cumbersome for use in total darkness and did not hold the film tight and flat. Small pinches of soft red wax were tried at each corner of the film and this method was found to be satisfactory. The film was held tight and flat on the drum and the camera could be easily and quickly loaded. The film used was specially prepared by Messrs Ilford, to whom my best thanks are due, and consisted of a special non-perforated film base coated with their hypersensitive panchromatic emulsion.

In order that the drum should only be exposed for one complete revolution it was necessary to calibrate the shutter, which was of ordinary iris type with time exposures marked $\frac{1}{2}$, $\frac{1}{5}$, $\frac{1}{10}$, etc. The calibration was easily made by measuring the length of film exposed at a known drum-speed. This gave the results shown in table 1.

Table 1

| Indicated time (sec.) | Actual time (sec.) |
|---|---|
| 0·500 | 0·820 |
| 0·200 | 0·330 |
| 0·100 | 0·165 |
| 0·085 | 0·133 |
| 0·070 | 0·097 |
| 0·055 | 0·075 |
| 0·040 | 0·064 |

This covers a range of drum-speeds from $1\frac{1}{4}$ to 16 r.p.s., intermediate speeds being assumed to follow a linear law.

Some traces with large amplitude and high film-speed are shown in figures 4 *a* and 4 *b*. These were taken with a film-speed of 30 ft./sec. They show how, with large amplitude, the presence of an harmonic of suitable frequency may excite a higher mode of vibration in the membrane which is superimposed on the main vibration. This is indicated by the curious doublings shown in the traces.

## § 8. PRODUCTION OF A CONTROLLABLE WAVE-FORM

What was desired was the ability to add to a fundamental sine wave the desired harmonic in any required proportions. After some preliminary experiments with other methods, it was found that exactly what was required could be obtained by coupling together two audio-frequency oscillators, one tuned to the fundamental and one to the harmonic, provided that the coupling was sufficiently close for synchronism to take place.

It is well known that vacuum-tube oscillators have an inherent tendency to synchronize with any oscillation of approximately their own frequency which may be present. Two factors influence the frequency-difference at which synchronism occurs—the strength of the injected current and the frequency-stability of the oscillator.

This phenomenon of frequency synchronization also occurs to a somewhat less degree between circuits tuned to frequencies bearing integral ratios to one another. It was this property which was utilized. For this purpose use was made of two oscillators. The schematic arrangement is shown in figure 3 *a*. A fundamental was supplied from a Sullivan oscillator which, for convenience, will be referred to as *oscillator A*. It gave a note of very pure wave-form and was of high frequency-stability. Oscillations from oscillator *A* were injected at a suitable point into another oscillator, of lower frequency-stability than oscillator *A*, and tuned to the desired harmonic. This latter oscillator we shall refer to as *oscillator B*.

It was necessary to find a suitable point for the feed-in to oscillator *B*. The grid bias of *B* is connected to the tuned circuit of the oscillating valve and thence to the grids of the amplifiers, figure 3 *b*. The desired effect was obtained by feeding-in oscillations at *A*, i.e. between the grid terminal and the negative of the bias battery. This caused synchronization of the oscillations in the tuned circuit of *B* and the combination was passed on to the amplifying system. The output from oscillator *A* was controlled by a 50,000-Ω. potentiometer *I*.

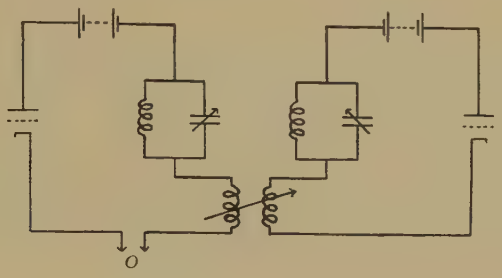



Figure 3 *a*.

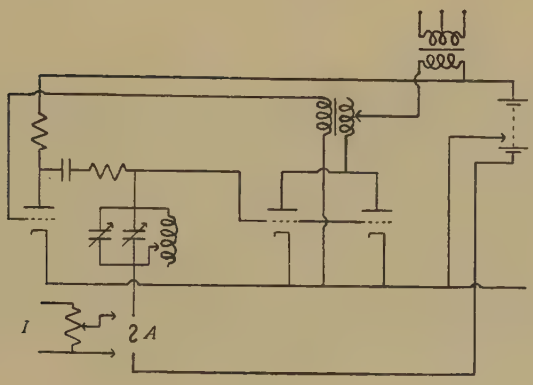



Figure 3 *b*.

The two oscillators were tuned as nearly as possible to, say, the fundamental and the 2nd harmonic, and switched on. In general, beats were heard in the hum of the output transformer and these became gradually slower as the volume-control on oscillator *A* was increased, until at a certain point the beats ceased and a uniform note was obtained showing that oscillator *B* had been pulled into synchronism. As far as was practicable, traces were used containing approximately equal proportions of fundamental and harmonic.

## § 9. METHOD OF TESTING CONSTANCY OF VELOCITY

The oscillators were adjusted to be in synchronism and to give a steady wave-form of the required type. The film-speed used was about 16 ft./sec.

A record of the wave-form at two different distances from the source was required for each frequency. The maximum convenient distance was about 325 cm.

and the minimum 75 cm., giving a distance change of 250 cm. Traces obtained ve close to the receiver were inclined to be unstable and to show varying amounts high harmonics.

If the volume control at the two distances were kept the same, then it was foun that the range of amplitude obtained was too large for adequate measurement. T avoid this, the volume of sound was adjusted when the distance was varied so a to make the amplitude recorded on the film the same at both distances. Then a suitable constant distance having been chosen, the variation with volume ove the above range was investigated.

Thus for each frequency investigated we obtain four traces, which we have t analyse so as to obtain their phase relations. We find that measurements of the bes traces can be repeated to 0·5 per cent. It is safe to say that the general error i measurement will be of the order of 1 per cent.

We have two functions

$$f_1(t) = a_0 + a_1 \cos \omega t + a_2 \cos 2\omega t + \ldots\ldots + b_1 \sin \omega t + b_2 \sin 2\omega t + \ldots\ldots,$$
$$f_2(t) = a_0' + a_1' \cos \omega t + \ldots\ldots\ldots\ldots + b_1' \sin \omega t + \ldots\ldots,$$

which represent the two traces on the film.

We measure $2\rho$ ordinates per period $A_1, A_2, \ldots\ldots A_{2\rho}$ and $A_1', A_2', \ldots\ldots A_{2\rho}'$ and hence deduce two functions

$$\phi_1(t) = \alpha_0 + \alpha_1 \cos \omega t + \alpha_2 \cos 2\omega t + \ldots\ldots + \beta_1 \sin \omega t + \beta_2 \sin 2\omega t + \ldots\ldots,$$
$$\phi_2(t) = \alpha_0' + \alpha_1' \cos \omega t + \ldots\ldots\ldots\ldots + \beta_1' \sin \omega t + \ldots\ldots,$$

which pass through the measured points. Since $A_1$, $A_1'$, etc. are only measured to 1 per cent, measurements of the values of $a_n$, $b_n$ cannot claim an accuracy greater than 1 per cent.

But another source of error arises in that although $\phi_1(t)$ and $\phi_2(t)$ pass through the given points we have no knowledge as to their behaviour between those points. In other words the $\alpha$s and $\beta$s are determined by a finite summation whereas the true $a$s and $b$s are integrals.

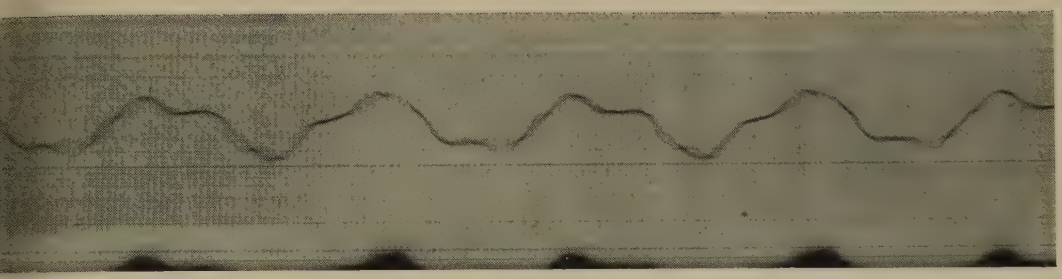
The deviation of the $\alpha$s and $\beta$s found by analysis from the true Fourier constants is given by

$$\alpha_1 = a_1 + a_{2\rho-1} + a_{2\rho+1} + a_{4\rho-1} + a_{4\rho+1} \ldots\ldots,$$
$$\alpha_2 = a_2 + a_{2\rho-2} + a_{2\rho+2} \ldots\ldots,$$
$$\vdots \qquad \vdots$$
$$\alpha_n = a_n + a_{2\rho-n} + a_{2\rho+n} \ldots\ldots.$$

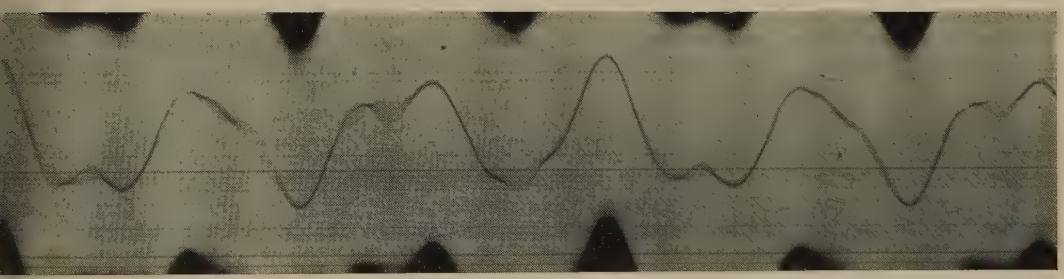
Similarly

$$\beta_1 = b_1 - b_{2\rho-1} + b_{2\rho+1} \ldots\ldots,$$
$$\vdots \qquad \vdots$$
$$\beta_n = b_n - b_{2\rho-n} + b_{2\rho+n} \ldots\ldots.$$

In the present work the amount of high harmonic is small since the wave is an addition of two good sine waves. It will be shown that the amplitude of the 21st harmonic is less than 0·05 per cent of the fundamental amplitude. We can therefore say that the $\alpha$s and $\beta$s will not differ from the true $a$s and $b$s by more than 1·5 per cent.



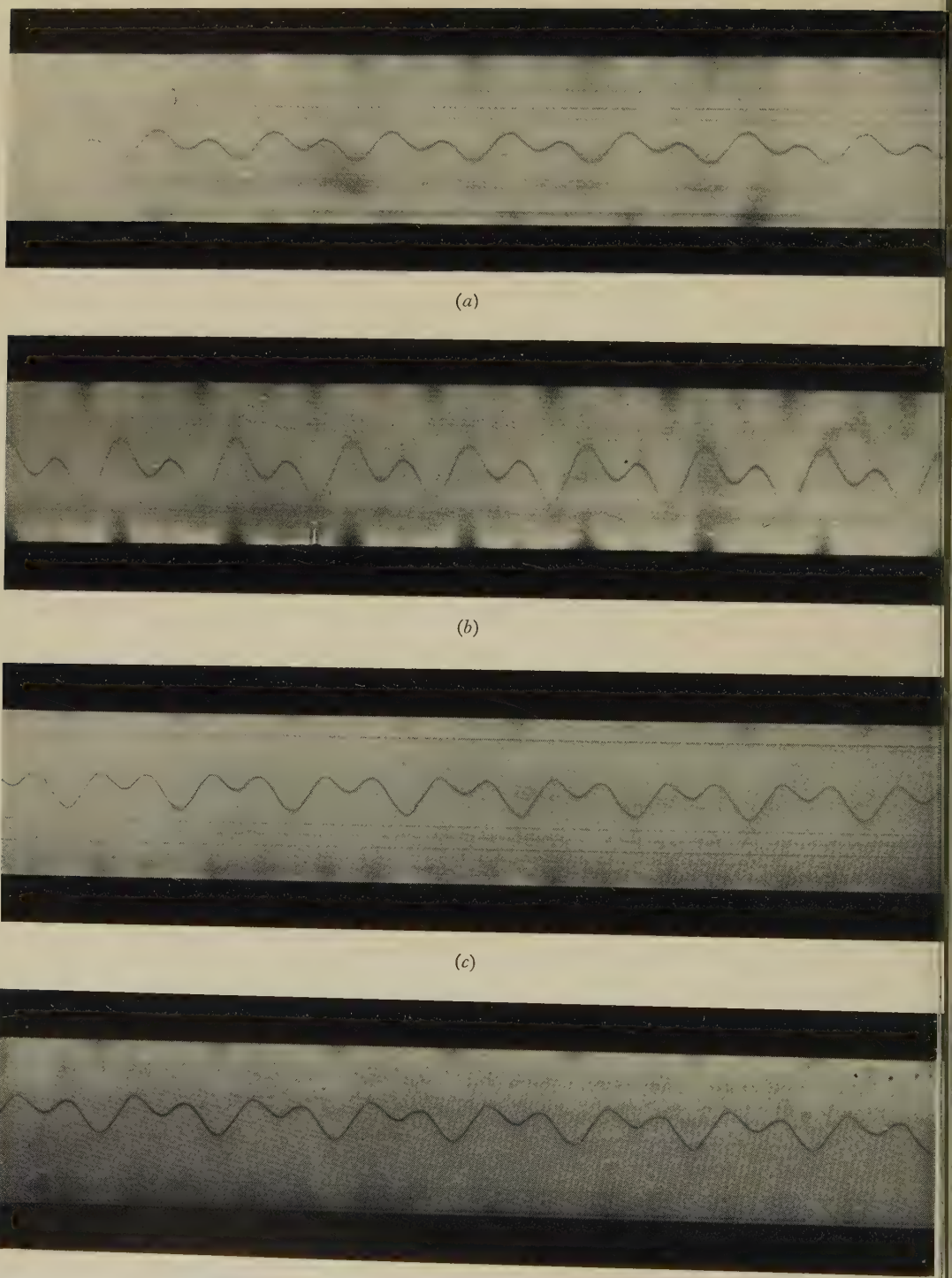
(*a*)



(*b*)

Figure 4.

*To face page* 910



(*a*)

(*b*)

(*c*)

(*d*)

Figure 5.

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It is easy to show that knowing the $\alpha$s and $\beta$s to 1·5 per cent then we also know the tangents of the phase angles, i.e. $\beta/\alpha$, to 1·5 per cent and so can detect a difference of 1° in the phase angle $\theta$ for all values of $\theta$.

The required amplitudes are calculated from

$$\sum_{q=0}^{2p-1} A_q \cos\frac{qn\pi}{p} = p\{a_n + a_{2p-n} + a_{2p+n} \ldots\ldots\}$$

and

$$\sum_{q=0}^{2p-1} A_q \sin\frac{qn\pi}{p} = p\{b_n - b_{2p-n} + b_{2p+n} \ldots\ldots\},$$

when $2p=24$ and terms $a_{2p-n}$, $b_{2p-n}$, etc. are accounted for above.

Ordinates were measured with a travelling microscope for five wave-forms at different points on the film and the mean ordinates were taken. This procedure took account of small variations in wave-form which sometimes occurred. Calculation showed that these variations were almost entirely due to small periodic amplitude-variations in the oscillator outputs.

## § 10. DISCUSSION OF RESULTS

The curves for which results are calculated for frequencies 250 and 500 c./sec. are shown in figure 5.

(*a*) and (*b*) show variation with volume at constant distance. Analysis gives

$$(a)\quad y_1 = 1{\cdot}12\cos\omega t + 1{\cdot}53\cos(2\omega t - 8{\cdot}1^\circ) + \ldots\ldots,$$
$$(b)\quad y_2 = 2{\cdot}00\cos\omega t + 2{\cdot}43\cos(2\omega t + 6{\cdot}9^\circ) + \ldots\ldots.$$

This gives a phase-change of 15·0° with the same variation of volume emitted as is used on a distance effect (i.e. at constant volume received).

(*c*) and (*d*) show traces for near and distant positions respectively:

$$(c)\quad y_3 = 1{\cdot}42\cos\omega t + 1{\cdot}33\cos(2\omega t + 69{\cdot}1^\circ) + \ldots\ldots,$$
$$(d)\quad y_4 = 1{\cdot}65\cos\omega t + 1{\cdot}67\cos(2\omega t + 86{\cdot}8^\circ) + \ldots\ldots.$$

(*c*) and (*d*) have a phase-difference of 17·7°, leaving 2·7° unaccounted for by change in volume-control.

From a previous calculation we have phase-change $\theta$ for a velocity difference 1 in 1000:

$$\theta = \frac{0{\cdot}36nfx}{c_1}.$$

Substituting $x=250$ cm. for all cases, and $n=2$, $f=250$, we have $\theta = 1{\cdot}4^\circ$. Therefore velocities at frequencies 250 and 500 c./sec. do not differ by more than 1 in 500.

Traces for 250 and 750 c./sec. give

$$(a)\quad y_1 = 1{\cdot}15\cos\omega t + 0{\cdot}73\cos(3\omega t - 114{\cdot}5^\circ) + \ldots\ldots,$$
$$(b)\quad y_2 = 2{\cdot}76\cos\omega t + 1{\cdot}26\cos(3\omega t - 107{\cdot}8^\circ) + \ldots\ldots,$$
$$(c)\quad y_3 = 1{\cdot}62\cos\omega t + 1{\cdot}03\cos(3\omega t - 40{\cdot}2^\circ) + \ldots\ldots,$$
$$(d)\quad y_4 = 1{\cdot}86\cos\omega t + 0{\cdot}91\cos(3\omega t - 30{\cdot}8^\circ) + \ldots\ldots.$$

These give a phase-change of 2·7°, which corresponds to a velocity-difference of 1 in 750.

(*c*) and (*d*) repeated give

$$(c') \quad y_3 = 1{\cdot}66 \cos \omega t + 1{\cdot}64 \cos (3\omega t - 75{\cdot}8°) + \ldots\ldots,$$

$$(d') \quad y_4 = 1{\cdot}33 \cos \omega t + 1{\cdot}16 \cos (3\omega t - 66{\cdot}2°) + \ldots\ldots.$$

These again give a velocity-difference of 1 in 750 between frequencies 250 and 750 c./sec.

As a matter of interest the small 2nd harmonics of (*a*), (*b*), (*c'*) and (*d'*) above were determined. These gave a velocity-difference between 250 and 500 c./sec. of 1 in 450.

The curves for 500 and 1000 c./sec. give a phase-change corresponding to a velocity-difference of 1 in 1000.

It was pointed out when the analysis was discussed that its accuracy depended on the amplitudes of high harmonics being small. To confirm this the 12th harmonic was calculated for various cases:

250 and 500 c./sec., near position: $a_1 = 1{\cdot}42$; $a_{12} = 0{\cdot}0003$.

250 and 750 c./sec., distant position: $a_1 = 1{\cdot}86$; $a_{12} = 0{\cdot}0007$.

500 and 1000 c./sec., large amplitude: $a_1 = 2{\cdot}94$; $a_{12} = 0{\cdot}0002$.

We can be certain that for these curves $a_{21} \ll a_{12}$, so that the assumption as to accuracy of the analysis is justified. The results obtained are given in table 2.

Table 2

| Fundamental frequency (c./sec.) | Harmonic frequency (c./sec.) | Upper limit to velocity-difference (parts per 1000) |
|---|---|---|
| 250 | 500 | 2·0 |
| 250 | 500 | 2·2 |
| 250 | 750 | 1·3 |
| 250 | 750 | 1·3 |
| 500 | 1000 | 1·0 |

It is not suggested that the above velocity-differences are real ones. The fact that these differences all have the same sign, i.e. are positive with increasing frequency, suggests that they are due to some factor in the experiment not taken into account. They may be due to interaction between source and receiver, which varies with distance, or to a phase-displacement which varies with amplitude in the receiver itself.

## § 11. CONCLUSION

Changes in the phase-velocity of sound in air with frequency have been determined by a direct method which is independent of the temperature and humidity of the air. The results tabulated above show that between frequencies 250 and 1000 c./sec. the velocity does not vary by more than 3 parts in 1000, and between frequencies 500 and 1000 c./sec. not by more than 1 part in 1000.

§ 12. ACKNOWLEDGEMENT

It gives me much pleasure to express my thanks to Prof. E. N. da C. Andrade for suggesting the problem to me, and for valuable assistance and advice during the work.

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## DISCUSSION

Prof. E. N. da C. Andrade. The reason that I suggested to the author that he should investigate the constancy of the velocity of sound at sonic frequencies was that Dr D. H. Smith and I, using a tube method, particulars of which will be published shortly, found in our first experiments a well-marked variation of velocity with frequency in the region 600 to 1200 c./sec. Remarkably enough, the values found by Grüneisen and Merkel, which fall in the region 1400 to 5000 c./sec., showed a variation which exactly fitted on to the curve expressing our results. Although we have since convinced ourselves that, as would be expected, this apparent variation of velocity is due to some feature of the tube method as generally applied, it seemed worth while to attempt to establish the constancy beyond doubt, especially as few reliable measurements of velocity appeared at the time to have been made at frequencies below 1200 or so. I hope that Dr Walch's work will lead in due course to the vanishing of the unconvincing band-heard-at-a-distance argument from our text-books.

Dr J. E. R. Constable said that in Germany speech from a loud-speaker had been found to be intelligible five miles away. This constituted a more delicate test than that afforded by orchestral music, which was mentioned in the introductory section of the paper.

Mr T. B. Vinycomb. Some years ago the B.T.H. Co. arranged a number of loud-speakers in a regular pattern on a high wall. I believe the object was to investigate whether distortion was introduced at great sound-intensities. They found that speech was clear and intelligible at distances of a couple of miles.

Author's reply. The details given by Dr J. E. R. Constable and Mr T. B. Vinycomb regarding the intelligibility of speech sounds at great distances are of interest and certainly constitute a more exact test than the one quoted from the elementary text-books. I am not aware, however, of any results which might be used to calculate how intelligibility would vary with dispersion of sound.

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# VIBRATION OF A BUILDING PARTITION AT AUDIO FREQUENCIES

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*ABSTRACT.* A simple device for observing the vibration of a partition has been used for determining the resonance frequencies and associated resistance coefficients of a sheet of window glass.

## § 1. INTRODUCTION

It has been shown[1,2,3,4,5,6] that there is a well-marked correlation between the sound-insulation and the weight per square foot of single sheets of widely varying material. Such a correlation would be expected if a simple partition could be regarded as a piston having elastic constraints so feeble that all its resonances were low compared with the lowest frequency at which measurements are made. The insulation calculated on this basis is, however, rather higher than the experimental figure, the difference increasing with the weight of the partition. Davis and Sabine explain this difference as due to the fact that partitions actually have resonances within the range of frequencies used for measurements. Davis also remarks that a series of resonances is necessary to account for the results.

It is of course to be expected that partitions will resonate at a number of frequencies, though these cannot be accurately calculated since the boundary conditions at the edge are so vague. It is therefore of interest to investigate them experimentally. It is difficult to deduce the resonance frequencies from measurements of the sound-transmission since pure tones would obviously be necessary, and errors would arise from interference effects. Accordingly the amplitude of the partition itself must be measured. E. Meyer[5] and A. H. Davis and T. S. Littler[7] have done this in the case of a brick wall and a building board respectively. Their measurements were, however, made at a set of selected frequencies and hence would not be expected to detect all the resonances of the wall.

In the following paper measurements made upon a sheet of window glass are described, the vibration being detected by an electromagnetic device. In contrast to the above-mentioned measurements, the frequency was varied continuously, its coincidence with a resonance frequency being shown by a sharp increase in the vibration of the glass.

## § 2. EXPERIMENTAL DETAILS

The measurements were made in the National Physical Laboratory's sound-absorbent transmission rooms. A sheet of 21-oz. glass measuring about 4 ft. by 4 ft. was clamped and sealed into the aperture which connects the two rooms and

was excited by pure tones from a loud-speaker directed obliquely towards it. Stationary-wave effects were minimized by the sound-absorbent character of the walls of the room.

To detect the vibration of the glass, light rectangular search coils constructed as in figures 1 *a* and 1 *b* were cemented to it. Attached to the centre of the sheet, coil *A* detects the majority of the resonances. There remain however a few resonances which have nodal lines passing through or near the point of the attachment. These can be detected by replacing coil *A* by coil *B*, used with its plane successively in two directions at right angles. The coils were rectangular in shape and each consisted of about 20 turns of fine insulated copper wire, wound as shown on a small piece of thin cardboard; they were too light to affect the vibrations of the glass appreciably. One side of each coil moved across the field of a permanent magnet, the pole pieces of which had an area of about $0{\cdot}75 \times 0{\cdot}75$ cm. The opposite side of the coil was well removed from the magnetic field. The voltages induced in the wire by its motion were detected after amplification by means of a copper-oxide rectifier and

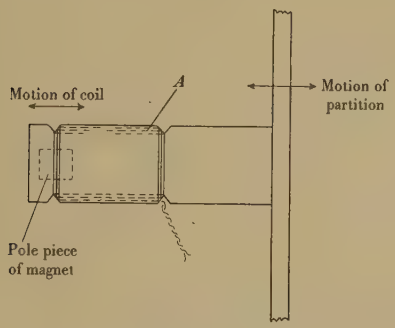


Figure 1 *a*.

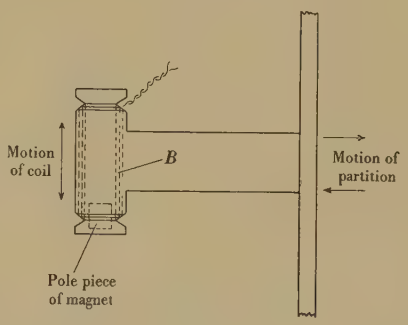


Figure 1 *b*.

galvanometer. To measure the output from the coil a comparison method was used, the e.m.f. from the secondary of a variable mutual inductance being substituted for the coil output. The coils were found to be sufficiently sensitive for examining the resonances of glass $\frac{1}{10}$ in. thick. By increasing the number of turns and the magnetic field, the sensitivity could no doubt be increased sufficiently for much heavier partitions to be dealt with. As the coils move in an approximately uniform magnetic field, their output is closely proportional to the amplitude of vibration. This property was used in determining the damping coefficients of the glass.

The resonance frequencies were determined very simply by varying the frequency of the loud-speaker note until the galvanometer deflection was a maximum. The frequency was then measured with a Dye sonometer[8]. For the most part the damping coefficients were determined from the frequency-changes, which reduced the amplitude as measured with the above described device to $\frac{3}{4}$ and $\frac{1}{2}$ of the resonance amplitude. In one or two cases, however, a complete resonance curve was drawn. At the low frequencies it was possible to obtain some check on the measurements by taking an oscillographic record of the decay of vibrations resulting from a blow. The search coil was in this case connected to the amplifier as before and the

amplifier output was connected to a Duddell oscillograph. Satisfactory agreement was found between the two measurements.

Errors in the measurements arise from the residual interference wave pattern in the room and the fact that the loud-speaker itself has resonances. The former effect is not likely to be noticeable except at low frequencies, since at other frequencies there would always be a number of nodes in the sound-field distributed over the surface of the glass, and change of frequency would only move some nodes on and a similar set of nodes off. The effect is in any case likely to be small owing to the absorbent character of the rooms. The fact that the same results were obtained when the loud-speaker was moved indicates that this source of error may be neglected.

Spurious peaks introduced by loud-speaker resonances are unlikely to have had much effect since roughly speaking the resonance peaks were 10 to 20 db. above the average vibration of the sheet, and of the loud-speaker resonances only the fundamental, at about 40 c./sec., was as large as this. In view of the non-resonant character of the cardboard used for the vibration-detector and the fact that its fundamental resonance was obviously very low, it is unlikely that any of the observed resonances had their origin in the detecting system, although the possibility cannot be definitely discounted. It appears justifiable however to assume that the majority, if not all, of the resonances observed were truly resonances of the glass sheet.

The stronger resonances and the vibration pattern associated with them could be observed by touching the glass lightly with the finger-tips or the knuckles. Nodes could very readily be traced in this way.

## § 3. RESULTS

The fundamental resonance of the glass as determined from an oscillogram of its vibration was 17 c./sec. This compares with the 17-c./sec. fundamental frequency calculated from Rayleigh's formula[9] for a circular plate with fixed edges, the same physical constants, and a diameter equal to a side of the square sheet of glass. Incidentally, the frequencies of the first ten overtones of such a disc coincided within 1 per cent with observed resonances of the glass sheet. The author is unable to say how much significance can be attached to this agreement, however.

About 95 well-marked resonance frequencies were observed below 3000 c./sec. Above this frequency the response had become so small that resonances could not be located with any accuracy. Plotted on a logarithmic scale the frequencies were approximately equally spaced, the geometric mean ratio between successive resonance frequencies being 1·055 : 1.

In the table are given the frequencies, resistance coefficients and sharpness of resonance of a number of the resonances observed.* These are selected on account of the large amplitudes associated with them.†

* The resistance coefficient given is the factor $k$ in the equation of motion:

$$\ddot{x}+k\dot{x}+n^2x=0,$$

and the sharpness of resonance is $2n/k$, the reciprocal of the logarithmic decrement.

† It cannot, however, be inferred that these were the principal resonances, since the position of the search coil must have been more favourable to some resonances than to others.

Table. Frequency, resistance coefficient and sharpness of resonance of some resonances of a sheet of 21-oz. window glass measuring 4 ft. by 4 ft.

| Resonance frequency (c./sec.) | Resistance coefficient (c.g.s. units) | Sharpness of resonance (c.g.s. units) |
|---|---|---|
| 17 | 5·3* | 40 |
| 72·5 | 6·9* | 132 |
| 96 | 6·9 | 75 |
| 107 | 13·1 | 93 |
| 136 | 10·9 | 156 |
| 157 | 38 | 52 |
| 164 | 14 | 147 |
| 240 | 25 | 120 |
| 293 | 43 | 85 |
| 708 | 93 | 96 |
| 737 | 130 | 71 |
| 817 | 146 | 70 |
| 869 | 173 | 63 |
| 2635 | 570 | 58 |
| 3240 | 600 | 68 |

* These damping factors were determined both from the resonance curves and from oscillograms. Satisfactory agreement was found and the figures given are the means of the two results.

## § 4. DISCUSSION OF RESULTS

It will be seen that while the general trend is for the resistance coefficient to increase with frequency, the sharpness of the resonance and hence the logarithmic decrement nevertheless remain of the same order throughout. It may be observed in this connection that as the vibration pattern becomes more complex so the internal damping may be expected to increase. On the other hand the damping due to sound-radiation may be expected to decrease, since a partition divided into a number of areas vibrating in opposite phase is an inefficient sound-radiator[10].

The radiation damping may be calculated approximately for the 17-cycle fundamental resonance since all parts of the sheet are in that instance in the same phase. The wave-length of the sound of the frequency in question is about 66 ft. and is therefore large compared with the sheet, which measured 4 ft. by 4 ft. Regarding the sheet of glass fixed in the transmission-room aperture as a circular rigid piston, of diameter equal to the side of the sheet and moving in a baffle, we have that the total radiation of energy from both sides is $a^2\rho\omega^2\dot{x}^2/c$ per unit area, where $\rho$ is the density of air, $c$ the velocity of sound in air, $\omega$ the pulsatance of the sound radiated, $\dot{x}$ the velocity of the piston's motion, and $2a$ the length of the side of the sheet. The mean rate of energy-radiation per unit area is accordingly $a^2\omega^2\rho\dot{x}_m^2/2c$, where $\dot{x}_m$ is the velocity amplitude. On substitution of numerical values, this gives

$$0{\cdot}9 \times \dot{x}_m^2 \text{ erg/cm}^2\text{-sec.}$$

The equation of motion of the sheet of glass can be written as $m\ddot{x} + k\dot{x} + n^2x = 0$, the average rate of energy dissipation being $\frac{1}{2}k\dot{x}_m^2$. Hence the resistance coefficient due to radiation is 1·8/0·68 or 2·6, since the glass weighed 0·68 g./cm$^2$.

This figure is about half the observed figure, 5·3, and is of interest as showing the large part played at the fundamental resonance by radiation damping. It does not appear to be possible to calculate the damping associated with the other modes of vibration.

§ 5. ACKNOWLEDGEMENTS

The author expresses his gratitude to Dr G. W. C. Kaye, Superintendent of the Physics Department, National Physical Laboratory, for his interest in this work and to Dr A. H. Davis for helpful criticism.

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## DISCUSSION

For the discussion see p. 922.

534.833.1

# THE VIBRATION PATTERN OF A WALL TRANSMITTING SOUND

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*ABSTRACT.* The vibration pattern of a 9-in. brick wall transmitting pure and warble notes of various frequencies has been determined by exploring the surface of the wall with an electromagnetic device. The pattern increases in complexity with increase of frequency. The effect of a warble tone is to decrease the difference between the greatest and least vibration energies.

MEASUREMENTS made in a number of laboratories[1,2,3,4,5,6,7,8] have confirmed the existence of a correlation between the weight of a single homogeneous partition and its sound-insulating value. In fact if the sound insulation is measured logarithmically (e.g. in decibels) the relation between sound-insulation and the logarithm of the mass per square foot is very nearly linear.

Several attempts have been made to calculate the sound-insulation of a simple partition[1,9,10] but the formulae obtained, while they agree with experiment for light-weight partitions such as plywood and building board, give values for heavy partitions which are too high. Davis[1] points out that this can be explained by the resonant properties of such a partition but finds that more than one resonance is necessary to account for the discrepancy. The existence of a number of resonance frequencies for partitions has already been confirmed[11,12]. This is to be expected, of course, if the partition can be regarded as a vibrating plate clamped at the edges. One would expect the partition to have a series of nodal lines similar to Chladni's figures. It is in fact possible, if a thin partition such as a sheet of glass is vibrating at a resonance frequency, to trace the vibration pattern with one's finger-tips.

It is of interest to attempt to trace the vibration pattern of a thick wall under the conditions which obtain when its insulation is measured. A brick wall 9 in. thick, plastered on both faces and built into the test aperture in the large sound-transmission rooms[13] at the National Physical Laboratory, was used for this purpose. The wall had an area of about 10 ft. by 8 ft. Sound was generated in the room on one side of the wall, and on the other side the amplitude of vibration of the wall was measured at a number of positions distributed over its surface. To measure the amplitude a heavy moving-coil loud-speaker movement was used, a brass rod attached to the moving coil being pressed against the wall. The alternating voltage

induced in the moving coil by the vibrations conveyed to it via the brass rod was measured with a calibrated amplifier.

Measurements were made at frequencies of 100, 200, 300, 500, 700, 1000, 1600, 2000 and 4000 c./sec. At each frequency both a pure note and a warble note were used, the frequency in the latter case varying cyclically by $\pm 20$ per cent up to 1000 cycles and thereafter by $\pm 100$ cycles. As a change of the test frequency would be expected to alter the vibration pattern, special care was taken to maintain the frequency constant. A heterodyne oscillator having good frequency-stability was used, the frequency-drift being not greater than about one cycle per hour, and the frequency was rechecked at intervals of about 10 minutes. It was probably maintained constant during the measurements to about $\frac{1}{5}$ c./sec. The adequacy of these arrangements was confirmed by the fact that measurements made at the same test frequency on different days agreed closely.

The number of observation points should obviously be as great as possible, but considerations of the labour involved limited the measurements to 63 points separated by 1 foot over a rectangle measuring 8 ft. $\times$ 6 ft. The need for measurements near the edge of the wall, where conditions are somewhat indefinite, was thus avoided. From the measurements obtained, contour maps were sketched, neighbouring contour lines corresponding to differences of 5 db. in the energy of wall-vibration. A selection from the figures thus obtained is reproduced in figures 1 and 2, in which the areas between the contours are shaded, the darker shading signifying the strongest vibration. The figures are arranged in pairs showing the vibration pattern with pure and warble tones respectively.

The fundamental frequency of the wall, as determined from a record of its free vibrations, was about 65 c./sec. This is outside the range of the frequencies used for the measurements.

It will be noticed that a pattern is visible in all cases, though the patterns do not exhibit the symmetry of Chladni's figures. Three factors may be responsible for the lack of symmetry, viz. the indefinite conditions at the boundaries of the wall, the inhomogeneous nature of the wall, and the existence of interference patterns in the air-borne sound which excites the wall. Owing to the last-mentioned factor, the sound pressure over the wall will vary from point to point and might give rise to areas of minimum vibration on its own account.

The maxima and minima are well marked whether a pure or a warble tone is used. For pure notes the difference between the greatest and least vibration-energies is 12 db. at 100 c./sec., 34 db. at 500 c./sec., 35 db. at 1600 c./sec. and 22 db. at 4000 c./sec. The differences were somewhat less when a warble tone was used, being 13 db. at 100 c./sec., 12 db. at 500 c./sec., 21 db. at 1600 c./sec. and 16 db. at 4000 c./sec. In the corresponding case of the interference pattern in the air-borne sound in the room, with pure notes the maximum energy-differences were 9 db. at 100 c./sec. and 19 db. at 4000 c./sec., whereas with warble notes the differences were reduced to 2 db. at 100 c./sec. and 3 db. at 4000 c./sec. It appears therefore that the patterns observed on the wall are not greatly influenced by interference patterns in the air-borne sound which drives the wall.

100 *c./sec., pure tone*

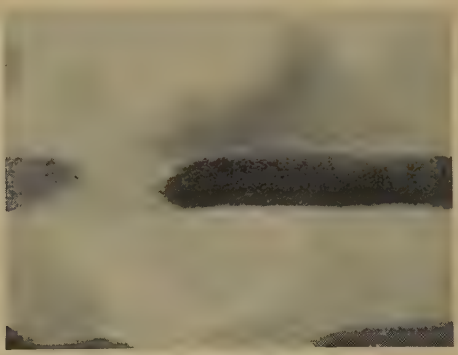
100 *c./sec., warble tone*

500 *c./sec., pure tone*

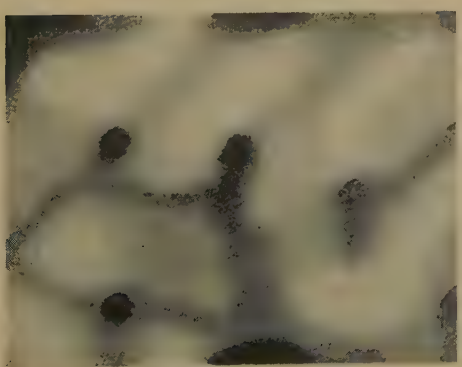
500 *c./sec., warble tone*

Figure 1. Vibration pattern of plastered 9-in. brick wall at frequencies of 100 and 500 c./sec. Areas of greatest vibration are shown black.

1600 *c./sec., pure tone*



1600 *c./sec., warble tone*



4000 *c./sec., pure tone*

4000 *c./sec., warble tone*

Figure 2. Vibration pattern of plastered 9-in. brick wall at frequencies of 1600 and 4000 c./sec. Areas of greatest vibration are shown black.

The authors express their gratitude to Dr G. W. C. Kaye, Superintendent of the Physics Department, and to Dr A. H. Davis for their interest in this work, to Messrs R. Berry and W. C. Copeland, who assisted with the measurements, and to Miss V. M. Fox, who prepared the figures.

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## DISCUSSION ON THE PRECEDING TWO PAPERS

Mr T. Smith. Does the indefiniteness in the boundary conditions which prevents mathematical solution affect the general applicability of the experimental findings: that is, to what extent does variation in the constraints imposed at the edges of a block produce significant changes in the properties examined?

Prof. E. N. da C. Andrade. What exactly was the method of clamping adopted, and could consistent values be obtained if the plate was dismounted and then reclamped? I am particularly interested to note the large number of resonances, at more or less equal spacing on a logarithmic scale, found by Dr Constable. When carrying out experiments on a sensitive water-into-water jet, subjected to sound waves over a very wide range of frequency, I observed selective response at a large number of frequencies, spaced more or less as those described by him. The lowest response was, however, at a frequency of a few vibrations a second. This behaviour was originally attributed to some property of the jet, but was eventually traced to resonant response of the structure of the room. I attributed it mainly to resonances of the floor, and Dr Constable's experiments confirm my belief in the correctness of this attribution.

Dr L. E. C. Hughes. The discrepancy between the theoretical calculation of the loss through a partition, based on its mass, and the measured value might be explained by the wave transmission of energy which is not reflected at the incident surface on account of change of impedance and which accordingly generates sound on the far side apart from the motion of the mass as a whole, and thereby gives a diminished loss. Have the authors calculated the magnitude of the wave-transmission component?

In the technical application of springs for reducing the force applied to foundations arising from unbalanced forces within an engine, it is found that the low damping in the material of the spring permits the wave transmission of high-frequency components, although the spring is adequate for attenuating the forces of low frequency. To obviate this difficulty, damping-pads of cork or felt are used in addition to the springs, so that both high and low frequencies are catered for.

AUTHORS' reply. In reply to Mr T. Smith: The boundary conditions at the edge of a wall are in practice vague. The edges of the wall are not stationary, as can be shown by experiment; the other walls of the building which meet its edges are, in fact, not sufficiently rigid or massive to give a perfect clamped edge. Experiment also indicates, however, that the wall does not behave as if its edges were free. Accordingly, to calculate the resonance frequencies of such a wall is likely to be difficult. The problem is complicated by the lack of homogeneity of the wall. This makes it unlikely that the vibration patterns illustrated in this paper would repeat exactly for another wall built to the same specifications.

In reply to Prof. Andrade: Satisfactorily consistent results were obtained when the plate was dismounted and reclamped. The fundamental frequency of the glass sheet was 17 c./sec. The resonances above this frequency commenced at 26 c./sec. and thereafter increased in frequency by an average ratio of 1·055 : 1. The clamping was intended to imitate the conditions under which a sheet of glass is held in a window frame. For this reason the glass was plasticened to a wooden frame and the whole was clamped against the iron reveal of the sound transmission test aperture. The behaviour of the vibrating jet to which Prof. Andrade referred is very interesting. The resonances which affect it might be either those of the walls of the room (which usually range from 50 cycles upwards) or of the air, which embrace roughly speaking the same frequency range. We suggest that the latter is more probable.

In reply to Dr Hughes: The sound transmitted as wave motion through a partition was calculated by Rayleigh. The result coincides with that obtained for a rigid piston provided the piston is regarded as thin in comparison with the wavelength, i.e. that both faces of the piston are in the same phase. This form of transmission is, however, of minor importance since the critical angle for brickwork is of the order of 5°.

# OBITUARY NOTICES

## SIR JOHN CUNNINGHAM McLENNAN, K.B.E., F.R.S.

By JOHN SATTERLY

J. C. McLennan was born at Ingersoll, Ontario, Canada, on April 14, 1867 of Scottish parents. His early education was received in the schools of Ontario and at the University of Toronto, where he graduated in the mathematics and physics course in 1892. Appointed as an assistant in the department of physics, he soon forged ahead. Of a very energetic nature, he was responsible for the beginning of more than one university organization. He collected funds for the construction of the Convocation Hall of the University of Toronto, founded, with others, the Alumni Society, and started the undergraduate student newspaper.

He soon became widely known throughout Ontario as one of the leading scientists in the province. Quite early he showed his interest in research. He was one of the first in Canada to take X-ray photographs, he spent a year on his own financing at the Cavendish Laboratory, and later on journeyed to Germany. Returning to Toronto, he became professor of physics in 1904, was largely responsible for the building of the new physics laboratory, and became its director in 1907. The physics laboratory in Toronto was very generously planned, though not as completely as he had intended. His foresight was well exemplified in the organization of the building, and it has served well the needs of students and research workers up to the present time, when once more Toronto finds that an additional building is necessary if it is to maintain its front rank in the teaching of physics and in modern physical research. By practically allocating different floors to different colleagues, each in charge of some particular branch of physics, he encouraged each associate to build up his laboratory in his own way and to take a pride in it. In this way McLennan was released from the innumerable details of routine that sometimes nearly break the hearts of laboratory teachers.

His research work was interesting and many-sided. He had the intuition to judge what line of research was most promising and offered the better plums, and threw himself into it with great vigour. He was a born organizer and administrator, and he would collect a number of promising young men and, never stinting himself, he would drive them also as hard as they could go. The University of Toronto ably helped him with funds for the necessary apparatus, and he got his outside friends and wealthy organizations to supplement the university grants; the modern system of research scholarships and fellowships brought him the men.

In his early days he practically worked night as well as day, and although, as has been mentioned above, he worked his research men hard, they rarely regretted it, for he remembered them when they left, and many now hold university and other public posts which they owe largely to his enthusiasm on their behalf.

McLennan's earliest work was on gaseous ionization and radio-activity, and particularly on penetrating radiation, which he studied for many years, carrying out experiments on the ice of Lake Ontario in weather sometimes below zero. This work was followed by many years of spectroscopic research. Hilger's found him a good customer, and his beautiful papers enhanced the firm's reputation for accurate graphs. He and his co-workers wrote many papers on line spectra and spectrum series, on the spectra of gases in different states, and on ionization potentials. One particular research which brought him into prominent notice was that on the origin of the bright green line in the spectra of the aurora and the night sky. His explanation of this line excited much controversy, but was substantially correct. His views were well stated in the Bakerian Lecture to the Royal Society in 1928 on "The Aurora and its Spectrum".

An interlude in his spectroscopic researches occurred during the great war. In 1917 he was asked by the Admiralty to make a survey of Canada and look for helium in the natural gases—the helium was to be used because of its non-inflammability in balloons at the Front. The survey being completed and the results communicated to the Admiralty, he decided to go over to England and offer his services to that body on all scientific matters coming within its ambit. He pushed the project for the manufacture of helium, and his colleagues left in Toronto established factories in Hamilton and Calgary, and before the end of the war helium was actually produced and compressed in cylinders for shipment. But the Admiralty was parsimonious, and the United States having come into the war, the same project was pushed by them with such great vigour and enormous success that the centre of helium-production was shifted thither, and before the war was over supplies of helium from Texas were on their way to England.

Meanwhile McLennan and a band of workers which had gradually filtered over to him from Toronto and elsewhere were engaged on other problems, one of the chief being that of the detection and elimination of submarines. McLennan's pushfulness and disregard for red tape must have often surprised the routine-ridden officials at the Admiralty, but he found a great admirer—the admiration was mutual—in Sir John Fisher, and between them McLennan's work gave very satisfactory results. In fact, McLennan is often regarded in his homeland as the man who won the war. After the war he was invited to stay in England as scientific adviser to the Admiralty, but he preferred to be his own master, and that fact, combined with the love he bore his own university and laboratory, decided him to return to Toronto. Arriving back he found himself—again owing to his own foresight and energy—the possessor of a large quantity of helium. He himself tells how he found it just unloaded upon the docks at Liverpool and decided that it should return back across the Atlantic to the continent whence it had come, and he personally attended to its being put aboard the next west-bound steamer. Fired by the startling success of the work of Onnes at Leyden, he imitated Onnes, built a cryogenic plant in Toronto, again got a group of men at work and soon achieved success in this new field. He liquefied helium in 1923, and many in Toronto well remember his Royal Canadian Institute lecture in which he showed the Onnes experiment of the persistent current in the helium-

cooled lead ring, an experiment he was to repeat later to an admiring audience in the Royal Institution with helium dramatically brought to London by an aeroplane from Leyden.

McLennan was an ideal lecturer. He gave much consideration to what he had to say and said it well. His slides were always in order. He had a good presence and an engaging smile. He took his audience into his confidence and so carried them on that they thought they were part of him and carrying on the good work of research. He had a keen sense of the dramatic at lectures, especially public lectures. About 20 years ago he and his associates were measuring the residual ionization in gases. Lake Ontario is fairly free from radium, and to get rid of active impurities in the vessel containing the insulated rod whose electrical leak was being measured he made his ionization chamber of lake ice. To get rid of the effect of the penetrating radiation from the surroundings he sent his men to take the readings out on the ice in Toronto Bay on a night when the temperature was down near zero Fahrenheit. That night he was lecturing to the Royal Canadian Institute on residual ionization and penetrating radiation, and after a discussion of the issues involved he said "The men are down on the ice to-night. I expect their results at any minute." At that instant a knock occurred on the lecture-room door. The door being opened, in walked two men dressed as if for Arctic exploration. One of them silently handed McLennan a sheet of paper. There was a great hush, followed by a dramatic gesture from the Professor. "Ladies and Gentlemen, the ionization is down to 2·1 ions per c.c. per sec." Everybody in the room thought they had taken part in an important experiment, and the clapping was tumultuous.

McLennan was a great believer in educating the public in scientific matters and getting their support for scientific projects. Quite early elected a Fellow of the Royal Society of Canada (1903), he became later the President of the Mathematics, Physics and Chemistry Section (1910), President of the whole Society in 1924, and Flavelle Medallist in 1926. He was one of the strong men, and sometime president, of the Royal Canadian Institute in Toronto. He lectured up and down the country on the metric system. It was about the only thing he couldn't persuade the people to adopt. He was put on the Advisory Council for Scientific and Industrial Research in Canada in 1916 and did active work there until his retirement. He educated the country to give money to research and to become reconciled to the expenditures involved in a National Physical Laboratory and the scientific departments of the Government. He was responsible more than anyone else for the building of the large telescope at Victoria, B.C. By helping every other scientific worker, he prepared the way for their support when his own projects appeared. He was very much in demand as chairman, for he had an ideal way of presenting a subject and such was his good nature and forcefulness that few could withstand him when he got going. In his last few years in Toronto he became interested in the application of radium to the cure of cancer and was the most prominent member of the Ontario Commission which toured Europe and collected the necessary data for the report afterwards adopted and acted on by the provincial government.

He was a frequent visitor to the British Isles and the Continent. In his early

days he did this at some financial strain, but after his marriage things became easier and his annual trips brought him into contact with all the leading men of the sciences in which he was interested. He often spoke of the great kindness shown him by Professor Oliver Lodge on his first trip to England, and recurring visits enhanced his good opinion of England and Scotland. He spoke sometimes as if he were a native of Aberdeen. He was a regular attendant at the British Association and was largely responsible for raising the money and making such a success of the 1924 meeting in Toronto.

He delivered the Guthrie lecture to the Physical Society of London in 1918 on the origin of spectra. His Presidential Address to Section A of the British Association at Liverpool in 1923 was on the same subject, and many will remember the packed lecture hall and the admirable summary of the subject which he gave. In 1917 he was elected a Fellow of the Royal Society, but apparently it was not until 1925 that he became a Fellow of the Physical Society. As has been recorded above, he gave the Bakerian Lecture in 1928 on the aurora and its spectrum. The award of the Royal Medal in 1927 for his spectroscopic researches further increased his attachment to the old country. The physics department under his regime used books and apparatus made in the old country as much as possible. In 1931 he was made Dean of the School of Graduate Studies in the University of Toronto, but a year afterwards, largely owing to the ill-health of his wife during the Canadian winters, he resigned his positions as professor and dean and retired to England, where he built a house for himself at Virginia Water. But he could not remain inactive. A laboratory was furnished in his house and he was again soon at work.

Being so near London, he often attended the meetings of the Royal Society and was a regular frequenter of the Athenaeum. The Royal Society elected him to its Council, made him Vice-President, and trusted him, as an impartial visitor, to administer its scholarship funds, and as a man of means to attend on its behalf the many scientific conventions on the Continent where it was necessary to have a first-rate man to make clear the position of the Society and through it that of the British Government. He was a supporter of many societies doing public work, such as the Royal Society of Arts, and was in great demand as a lecturer on low temperatures and superconductivity. His Kelvin lecture to the Institution of Electrical Engineers was a notable achievement, and so was his address at the International Conference on Physics in 1934.

Following up his Ontario experience of work on the relief of cancer by the use of radium, he became in England the Director of the Cancer Research Hospital in Riding House Street. He was enthusiastic over the cure of pain, and thanks to his good relations with the Belgian Radium Company he became the temporary possessor of the largest single supply of radium, and was able to push forward the bomb method of radium treatment.

McLennan married in 1910 Elsie M. Ramsay, a native of Toronto of Scotch descent and at that time living at Bowland, Stow, near Galashiels. They formed a very happy pair; he could never do too much for her, and no doubt her kindness and advice and moderating influence were of the greatest service to him, for in his early life

he was impetuous and often said and did things on the spur of the moment that upset his opponents, whereas in his latter days he became milder and kinder. In a way, however, he never left off being a boy and was always happy when his old students crowded around him and they revived the memories of ancient fights.

Mrs McLennan died in 1933, and McLennan never recovered from his loss. When he returned to Toronto next spring in his capacity as visiting professor he gave to the physics department portraits of himself and his wife and made a most moving speech praising her virtues and all that she had been to him.

McLennan received many invitations to be professor elsewhere, and even to be chancellor of one University, but he preferred to stay in Toronto. He was the recipient of honorary degrees from Toronto, Manchester and Liverpool. It is no secret that he would have received a knighthood just after the war if the Canadian Government had not prohibited Canadians from being thus honoured by the King. As it was, it was not until after the embargo was lifted that the King was enabled to honour McLennan with a knighthood. Unfortunately McLennan did not long enjoy his honour, for although he was apparently in good health and attended the British Association at Norwich in September and a congress in Paris in October, he fell ill suddenly on the way back from Paris on October 9, and died before medical assistance could be obtained.

He will be long remembered. On his retirement from Toronto the University decided to name the laboratory in which he had done such good work the McLennan Laboratory. Many years ago his father-in-law endowed the Ramsay scholarship for the best student in practical physics. McLennan himself by his will has endowed another scholarship.

In Canada in general and in Ontario in particular his memory will be kept green by his old students, many of whom occupy important posts which they owe to his continued interest, by his colleagues, and by the great mass of people who for so long regarded him as one of themselves who had lifted himself up to do great things.

## JOHN PARKE KIRKMAN

By C. W. HANSEL

Mr John Parke Kirkman died on December 15, 1935 in his 79th year. He was educated at Highgate School and Emmanuel College, Cambridge. He was a Scholar at King's College, London, before going to Cambridge. He had been a life member of the Physical Society since 1878. After a period as Headmaster of the Oxford Military College, he joined the Mathematical Staff of Bedford School in September 1893 and retired in July 1919. From his kindness of heart, discretion, and sanity of judgement, he exercised a beneficent influence not only in the form room but in the boarding house. For some years he was Principal of the Bedford Technical Institute and subsequently Chairman of its Executive Committee.

Few men have given more generously of their time and talents to the public service and particularly to the interests of scientific education than Mr Kirkman. He was a friend and admirer of the late Sir George Greenhill, and his outlook on mathematical teaching was essentially a practical one. He taught much of his mathematics in the laboratory and regarded practical mathematics as a laboratory subject which embraced all branches of practical science to which mathematics could be applied, such as practical physics, applied electricity, and engineering. He was untiring in his efforts to obtain laboratory accommodation and equipment not only in his own school, but in all schools.

It is impossible in this short notice to give an adequate idea of his work for the public service. He joined the Bedford Town Council in 1920. He was elected an Alderman and for a period acted as Deputy Mayor. He served as Chairman of the Housing Committee, Chairman of the Education Committee, and Vice-Chairman of the Public Health and Watch Committees. He was a member of the Bedfordshire Education Committee and served for a time as Chairman of the Higher Education Sub-Committee and Chairman of the Library Sub-Committee. He was appointed a Magistrate for the Borough of Bedford and for some years sat as Chairman of the local Juvenile Court.

He was appointed a Governor of the Harpur Trust by the Senate of Cambridge University. He was a member of the Estate and Finance Committee of the Harpur Trust and Chairman of several Harpur Trust Standing Sub-Committees. He acted as Chairman of the Schools Committee of the Harpur Trust.

Mr Kirkman was one of the founders of the Bedford Charity Organization Society. He was a life trustee of St John's Hospital Trust, and a trustee of the General Municipal Charities. He was a frequent, kindly, and sympathetic visitor to the old people in the Almshouses, and was a good friend to the Society for the Welfare of the Blind. There is no section of the community in Bedford that will not feel his loss.

## SIR RICHARD TETLEY GLAZEBROOK

### September 18, 1854—December 15, 1935

F.R.S., 1882; Assistant-director of the Cavendish Laboratory, 1891–8; Principal of University College, Liverpool, 1898–9; Director of the National Physical Laboratory, 1900–1919; President of the Physical Society, 1903–5; President of the Optical Society, 1904–5 and 1911–12; President of the Faraday Society, 1911–13; President of the Institute of Physics, 1919–21; President of the Institution of Electrical Engineers, 1906; Zaharoff Professor of Aviation and Director of the Aeronautical Department of the Imperial College of Science and Technology, 1920–3; Hughes Medal (Royal Society), 1909; Royal Medal (Royal Society), 1931; Foreign Secretary of the Royal Society, 1926–9; Gold Medal (Royal Society of Arts), 1917; Reid Lecturer, 1917; Guthrie Lecturer, 1931; Gold Medal (Royal Aeronautical Society), 1933; Knighthood, 1917; K.C.B., 1920; K.C.V.O., 1934.

The death of Sir Richard Glazebrook removes from our midst yet another of the slender band of veterans who came under the direct influence of Clerk

Maxwell, and who can tell us something of those early days when men plunged light-heartedly into exact physical research, their minds trained by the subtleties of the Mathematical Tripos, their hands by some practice at the bench of the village carpenter. Such was Glazebrook's training—and there is much to be said in its favour.

The son of a Liverpool doctor, he received his early education at Liverpool College, coming up to Trinity in 1872, shortly after Maxwell's appointment to the Cavendish Chair. It was a period of most active, and delightfully unsystematic work. Examinations under the new schedule, which recognized, tardily enough, the existence of such branches of science as Heat, Electricity and Magnetism, were instituted in 1873, and the Cavendish Laboratory (furnished, as Maxwell later optimistically remarked, "with all the instruments required in the present state of science") was opened in 1874. The shadow of Kelvin's quadrant electrometer loomed large in the physics of the seventies. We remember how Ramsay as a youngster at Glasgow was set, as his first exercise in practical physics, to taking the kinks out of a coil of copper wire, and then, having served this apprenticeship, was promoted to the study of the quadrant electrometer. So in this far-off Cambridge of sixty years since, we read (in tones slightly hushed) of the new Laboratory and "the Professor's private room, with the Quadrant Electrometer on a shelf in one corner".

Glazebrook himself, after graduating as fifth wrangler in the Tripos of 1876, went to the Cavendish Laboratory, on his tutor's suggestion, in search of a possible subject for a fellowship dissertation, and Maxwell set him to what we should nowadays regard as a very elementary piece of work, some Wheatstone's bridge measurements. Glazebrook has put on record his regret that he had not assimilated this knowledge earlier, for in the Tripos examination Rayleigh had set the question "Explain the Wheatstone's Bridge method of measuring resistances", and Glazebrook, fresh from the study of those portions of Maxwell's treatise which were *not* concerned with resistance measurement, found himself unable to touch both question and an easy rider appended thereto. After a little time spent on such measurements Glazebrook had to search for any possible variation in the electromotive forces of the Daniell's cells used by Chrystal in his experiments on the validity of Ohm's law, and the inevitable quadrant electrometer was pressed into service. Glazebrook, in common with most other physicists, found the instrument a trial, "perhaps a useful one", as he drily remarked. But measurements on Daniell's cells were not likely to lead to a fellowship dissertation, and Glazebrook found his subject in a careful and accurate test of Fresnel's wave surface in aragonite and in Iceland spar. The experimental work involved goniometric measurements of high precision, and was carried out under primitive and uncomfortable conditions. It shows that, taking into account the effects of dispersion, "Huygens' construction for the extraordinary wave-surface represents its true form to within about 1 in 30,000".

It is difficult in these days of organized laboratory classes of all grades and sizes to realize that systematic laboratory teaching, as we know it, is a product of the last

two generations. Rayleigh instituted practical classes in Cambridge with Glazebrook to assist in the organization of the system and with Napier Shaw as demonstrator. They started from "the foots", as the Lancashire phrase has it, and their experiences were remarkable enough. Was it about this time or a little later that there emerged the heroic figure of him who saw in a thermometer an instrument for the determination of specific gravities, though, as Wilberforce remarked in telling the story, "when furnished with a thermometer, a basin of water and a bit of string, he failed to achieve any numerical result"? At any rate, Glazebrook left his mark on the teaching in the laboratory, and the many editions of "Glazebrook and Shaw" testify to its almost world-wide influence. Of Glazebrook's other books his *Physical Optics*, now in a sense obsolete, is yet of value as giving a characteristically clear picture of the state of that science in the early eighties. His elementary text-books, the first editions of which are now some forty years old, still hold their own against more youthful competitors, and his biographical study of Clerk Maxwell contains one of the best summaries extant of Maxwell's teachings, and has, moreover, the advantage of having been written by one who had first-hand knowledge of his subject.

During this period Glazebrook's research work was moving in the direction of the tasks which dominated his later life, papers on optical subjects being more and more replaced by papers dealing with electrical standards and measurements. One of the earliest of these papers (1883) records an absolute determination of resistance by that method which consists in comparing the steady deflection, in a circuit possessing a mutual inductance which may be calculated accurately, with the inductive kick obtained on reversal of the battery current. The results of the investigation, reduced to their latest form, showed that

$$1 \text{ B.A. unit} = 0{\cdot}98665 \times 10^9 \text{ c.g.s. units.}$$

The series of such papers published previously to his appointment to the National Physical Laboratory ends with the investigation which describes the determination of $v$ made in collaboration with Sir Oliver Lodge. In this determination, a parallel-plate condenser, whose capacity in electrostatic measure could be accurately calculated, was placed in an inductive circuit of negligible resistance, and the capacity in electromagnetic measure was deduced from observation of the period of the oscillatory discharge and a knowledge of the magnitude of the self-inductance. The spark discharge was photographed on a plate which rotated at a speed of about 80 revolutions per second, and the period was thus determined with considerable accuracy. The capacity of the inductance coils used was a source of much trouble, and the final result ($3{\cdot}009 \times 10^{10}$ cm. sec.$^{-1}$) was not regarded as one of exceedingly high precision. The method is, however, one of considerable physical interest, and may be profitably used in a laboratory determination of $v$.

In 1898 he left Cambridge, having in the interval become Assistant-Director of the Laboratory and Senior Bursar of Trinity, to take up the position of Principal of University College, Liverpool. He barely assumed these duties to leave them, for in 1900 there fell to him his life's major task—that of Director of the newly formed National Physical Laboratory.

Glazebrook left his mark on many things and many movements—perhaps it was because in most of them the work had to be started, very much *ab initio*. It was so in his fellowship work, when two years of optical measurements produced papers of major importance from one who, at the beginning of the period, knew nothing of the Wheatstone's bridge; so, in his teaching work in practical physics; so, in the newly formed National Physical Laboratory, where buildings had to be erected, a staff brought into being, and a plan of campaign drawn out. In nothing are Glazebrook's remarkable capacities as leader and administrator more clearly shown than in his work at the National Physical Laboratory. In 1902, the two departments of physics and engineering and a staff of thirty sufficed for its modest needs—needs which rapidly grew with Glazebrook's growing experience. Building after building rose near Bushy House; metrology, electrotechnics and photometry, chemistry and metallurgy, were insistent in their demands for housing adequate to their needs; and all the while the growing requirements of the two original departments called for expansion in their staff, equipment and in the space allotted to them. The year 1911 saw the opening of the gigantic tank for experiments on ship-models, and aeronautical research was begun in 1909. Withal, as was to be expected in one of Glazebrook's outlook, those activities which are concerned with the improvement of standards of measurement in all branches of science, were never in the slightest danger of relegation to a subsidiary position. Not to confuse the record by considering in detail the stimulus afforded by the War years, it is sufficient to note that in ten years from 1902, the two departments and staff of 30, had expanded to six departments and a personnel of 150.

This, more than any other of Glazebrook's many achievements, is his enduring memorial; and of Glazebrook and the National Physical Laboratory it may be said, as truly as of Wren and St Paul's—*Si monumentum requiris, circumspice.*

Glazebrook laid down his duties as Director in 1919. Retirement to him meant no more than a shifting of his activities, for his was not the type that retires to cultivate its garden. He was happy in the opportunity of his death, for he worked at full pressure until his last day. At various times during these later years he was Director of the Aeronautical Department of the Imperial College of Science and Technology, Editor of the *Dictionary of Applied Physics* and Chairman of the Symbols, Units and Nomenclature Commission of the International Union of Pure and Applied Physics. All this work was stamped with the impress of his strong personality. The *Dictionary*, in particular, shows how serenely he could ignore that which made no appeal to him; for the *Dictionary*, wide as is its scope, contains not even a passing mention of colloid physics. Work on electrical standards played a great part in his life; such work, exact, shining perhaps with a dry light, and calling for great precision and clarity in definition, made a strong appeal to him; no better illustration of Glazebrook's mind and his meticulous scholarship can be found than in his Guthrie Lecture of 1931 on "Standards of Measurement, their History and Development".

Glazebrook's was an outstanding figure in the history of the physics of the past sixty years. Competent, exact, completely and easily master of the tools of the

physicist's craft, absorbed in the day's work—these are the phrases that rise in the mind as one's thoughts travel back to the committee rooms of the Royal Society; as one pictures again the tall, spare figure of the octogenarian striding into the room with the activity of one of half his years; hears the precise and curiously clipped diction; and sees the steel spectacles removed with a sudden, almost nervous jerk, and used to emphasize a point in the argument. That is the milieu in which one almost automatically places Glazebrook. Striking as was his work in physics, he will be remembered longest as a pre-eminently successful administrator. As such he will live long in the affectionate memories of those who have served under him, and not least affectionately in the memories of some of those who, on occasion, differed from (and with) him.

A. F.

# REVIEWS OF BOOKS

*Energy and Matter*, by CHARLES B. BAZZONI. *Space, Time and Relativity*, by H. HORTON SHELDON. (From The University Series, London: Messrs Chapman and Hall, Ltd.) Price of each volume 4*s.* 6*d.* net.

These two volumes are written with the purpose of presenting their respective subjects to readers who are not specialists but who seek a general acquaintance with the progress of modern physics.

The first volume traces the history of atomic theories, outlining the early views which prevailed before the discovery of the electron, and passing on to give, in approximately two-thirds of the volume, an account of the work of physicists since 1895. The author has produced a very interesting and a very readable work, and apart from providing the reader with accurate and plentiful detail he has succeeded in expressing the spirit of scientific research and the enthusiasm of the researcher. He discusses some of the changes in the point of view which most recent developments have brought in their train, and in a very short closing chapter he refers to comments of a metaphysical character which some modern writers have thought well to make, attaching to them just that quantum of importance which they deserve.

It is admittedly difficult to write a popular account of the theory of relativity, and the writer of the second of these volumes has set himself a difficult task in attempting to make clear to the layman the principles of Einstein's theory. Nevertheless he has succeeded in giving some idea of the viewpoint of the theory and of its important consequences. The illustration of the relativity of time, however interesting as a subject of thought, does not appear to have anything to do with the difficulty about simultaneity which the theory points out. Further, it is not worth while to bring into the discussion such abstruse questions as are involved in the theory of parallelism. Our chief adverse criticism is directed to the unscientific character of the work as a whole. It is as important to interpret the enthusiasm and the spirit of the scientific worker as to give the detail of his discoveries. The present work is too sensational. We are shown a page of an American newspaper with a few lines of one of Einstein's papers published in various forms and we are asked to marvel at the popular interest which made its publication good journalism. Again, in the illustration of the relativity of time the interstellar news delivered simultaneously everywhere is that of a murder. This sort of wonder is not the wonder of science. We are getting used to the complicated illustrations used by some writers and lecturers to convey an idea of the great magnitude of some of the numbers occurring in physics. This book is distinguished for the complexity and length of one of these in an attempt to show how large is the number $10^{63}$. If the American layman, for whom this book is primarily written, speaks as the English think he does, one can imagine that after reading it he might say: "I guess this is some number", and that, being a complete, precise and concise statement of fact, is more true to the spirit of science than a whole page of rigmarole. We come to the end of the book with a feeling of disappointment that a branch of physics has been brought into contact with the advertisement page and the sensational columns of a second-rate newspaper.

H. T. F.

*Anschauliche Quantentheorie*, by Dr Pascual Jordan. Pp. xii + 320. (Berlin: Julius Springer, 1936.) RM. 12.

This volume gives an admirable descriptive summary of the principal results obtained by the modern quantum theory and a brief history of the early development of that theory. If there is one criticism which can be made of the book, it is that the author has attempted to cover rather too much ground. In fact, the book discusses the quantum theory from every aspect—experimental, mathematical and philosophical. The first chapter gives a summary of the physical background of quantum physics, emphasizing specially the duality of the corpuscular and undulatory concepts of matter and indicating the physical significance of stationary states and quantization. The second chapter provides a theoretical analysis of the fundamental experiments, and discusses in detail the applications of the correspondence principle and the calculation of probabilities of transition.

The mathematical scheme of contemporary quantum theory is developed in the third and fourth chapters. These give a condensed sketch of matrix mechanics and of wave mechanics in which most of the principal results are stated, although limitation of space has prevented more than very brief sketches of the methods of proof. The fourth chapter deals specially with problems in which many particles are involved and also discusses the application of the quantum theory to the elementary articles of microphysics, viz. the alpha particle, the spinning electron, the nucleus, and the neutrino. The concluding chapter gives a very interesting account of the philosophical background of the quantum theory, indicating its relations to the general method of positivism and to the fundamental questions of causality and finality. The whole of this treatise is most useful and valuable as a work of reference—indeed, it is primarily as a work of reference and a useful summary of the subject that it will most probably be used. It is therefore especially regrettable that no index is included.

G. T.

*Foundations of Physics*, by R. B. Lindsay and H. Margenau. Pp. xiii + 537. (New York: Wiley and Sons. London: Chapman and Hall, Ltd.) 22*s*. 6*d*.

This book covers so much ground that a list of chapter headings and lengths is perhaps the best way of indicating its scope. The list is as follows: I. The meaning of a physical theory. (58 pp.) II. Space and time in physics. (20 pp.) III. The foundations of mechanics. (80 pp.) IV. Probability and some of its applications. (29 pp.) V. The statistical point of view. (92 pp.) VI. The physics of continua. (39 pp.) VII. The electron theory and special relativity. (37 pp.) VIII. The general theory of relativity. (31 pp.) IX. Quantum mechanics. (128 pp.) X. The problem of causality. (14 pp.)

The treatment is unusual and interesting. Certain sections of theoretical physics have been selected, and very judiciously selected, largely on the basis of their suitability as "material for exemplifying the basic structure of physical thought". A very concise and more or less conventional and familiar mathematico-physical treatment of these sections makes up the skeleton of the book; this in itself constitutes a useful connected summary of a great deal of moderately advanced physics, but its function here is the ancillary one of providing the framework for a full discussion of the foundations of the theories expounded. In other words, and in brief, the book is much more concerned with epistemology than with details of the mathematical technique of theoretical physics.

"The authors would like to believe that, among other things, they have made clear, to put it crudely, how mathematics gets into physics and how, moreover, certain mathematical methods are peculiarly appropriate for particular physical theories." In this they have been conspicuously successful, and they are to be congratulated upon the production of an original and inspiring book.

It is to be hoped that this work will be very widely read; it can be cordially recommended to all senior students of physics, and it will make a special appeal to a great many teachers of the subject.

H. R. R.

*Structure Factor Tables*, by KATHLEEN LONSDALE. Pp. 181. (London: G. Bell and Sons, Ltd., for the Royal Institution, 1936.) 10*s*.

The full title of this book is "Simplified Structure Factor and Electron Density Formulae for the 230 space groups of mathematical crystallography." The aim of the author as stated in the foreword has been to present tables of structure factors in a practical form, suitable for direct use. They differ from the general structure-factor equations included in the *Internationale Tabellen zur Bestimmung von Kristallstrukturen*, which cannot be applied without the further simplifications tabulated in this volume.

A full explanation of the way to use the formulae is contained in the introduction. The method adopted in the production of the volume, that of photographing the manuscript, precludes the possibility of printers' errors. The tables are clearly set out, are condensed to minimum space, and are easy to follow after a perusal of the directions which the author gives in the introduction.

The amount of work entailed in the preparation of the tables has been great. The author and the Royal Institution deserve the gratitude of all workers in X-ray crystal-analysis for preparing and publishing the tables so that they may be within reach of everyone interested in this important branch of modern physics. Every laboratory where work on crystal-analysis is being carried out should be in possession of this most valuable volume, which will undoubtedly be the means of curtailing much of the initial work necessary in problems involving structure factor and electron-density. And the price is exceedingly moderate. E. A. O.

*Heat for Advanced Students*, by E. EDSER, revised by N. M. BLIGH. Pp. x+487. (London: Macmillan and Co.) 6*s*.

The question is not whether this is a good text-book of heat (that has been answered by its 37 years of life) but whether the revision by a hand other than that of its original author has been successful. Such a revision must be very difficult. On one side is Scylla, represented by the temptation to alter the book out of recognition in the effort to bring it up to date, and on the other Charybdis, or the spirit of reverence which forbids the reviser to alter that which the author wrote.

The additions are numerous and mainly helpful. One may cite in particular the paragraph on the change of density with temperature, and the new description of thermometer-calibration, but the chief one, the introduction to the quantum theory, is so compressed that it is doubtful whether students at the stage when they normally read Edser will carry away any understanding of the utility of the theory. They will certainly have a confused idea that Nernst's theorem is some sort of consequence of the theory. (Incidentally, the functional $f$ which occurs on page 454 is almost enough to frighten the student off the subject; of all known symbols it most resembles Sommerfeld's sign for integration over a period.)

A certain number of wise deletions have taken place, but they are not sufficient, for it must be remembered that one of the outstanding features of Edser's book when it first appeared was the fact that it was so nearly abreast of the latest work. Had the author been writing it now, he would have described not Regnault's apparatus for vapour pressures at high temperatures but either that used at Munich or that used at Washington for the work on steam and carbon dioxide. He would also have described some more modern forms of thermal-conductivity apparatus, since they are actually easier to understand than the earlier forms.

Among omissions, we note the absence of any description of a total-radiation or an optical pyrometer, and the failure to make clear to the student that the Seebeck effect is

one which is widely used in factories and works for the measurement of temperature. There is no reference to the fact that the ventilated wet-bulb hygrometer exists.

Again (and it is to be remembered that all these complaints are only made because the book is so good that one expects to see many further editions, and wishes them to be even better), the numerical data nearly all need revision. On page 27 the boiling-points of water at different pressures are given to 0·0001° C., when the values differ by 0·004° from the recent results of Moser. On pages 239 and 464 we learn that the vapour pressure of steam at 40° C. is 54·87 mm. of mercury, whilst on page 353 it is 54·91, and in a second table on page 464 it is 55·55. The conductivity of aluminium on page 421 is wrong by about 50 per cent.

Finally, one may regret that one or two hoary old misstatements have been left uncorrected. Newton's law of cooling was never put forward as applying to radiation, as seems to be asserted on page 450, nor did Stefan say anything about black bodies when he announced his law. He deduced it, as stated, from observations by Dulong and Petit, on real bodies which were not in a black-body enclosure.

However long the above catalogue of blemishes may seem, it is to be taken, not as a sign of disapproval of the book, but rather as an indication that the latter is worthy of the most careful attention. Many a student will gain his first knowledge of the subject from it, and we all wish him to have a book as nearly perfect as possible.

J. H. A.

*Thermodynamics for Engineers*, by the late Sir J. A. Ewing. Pp. xv+389. 2nd ed. (Cambridge University Press.) 21*s*. net.

One of the outstanding characteristics of Sir Alfred Ewing was his lucid style of writing. He took meticulous care in the presentation of his material and his books are models of what scientific works should be as regards clarity of expression and literary style. Until the time of his death the revision of his book on thermodynamics had provided him with a congenial task which, alas, he was not destined to complete. Prof. Egerton has undertaken to complete the revision and readers will feel grateful to him for the care and thoroughness with which the work has been carried out. Prof. Egerton's work in connexion with the preparation of the steam tables brought him into close contact with Sir Alfred, so one has the assurance that the revision is in accordance with the author's views on the subject.

Sir Alfred had very decided preferences as regards the symbols for the thermodynamical quantities most used by engineers. The symbols he has adopted are in accord with those agreed by the International Union of Pure and Applied Physics, except that $\psi$ has been used for free energy and $F$ for electromotive force, while $G$ (Gibbs's function) is taken as $-G$, the positive value being more convenient for users of steam tables.

A committee of the Physical, Chemical, and Faraday Societies is now at work considering the symbols for use in thermodynamics, and whilst they have not issued their final report it appears probable that the letter $F$ will be used for free energy and that Greek letters will be avoided.

One minor point merits comment. It is stated on page 265 that "In triatomic gases it may be conjectured that the three atoms of any molecule group themselves not in one straight line—which would be an unstable arrangement—but so that the massive centres lie at the corners of a triangle". This statement needs modification, for it is now generally accepted that some triatomic gases, carbon dioxide for example, have a linear structure.

E. G.

*A Laboratory Manual of Experiments in Physics*, by L. R. INGERSOLL and M. J. MARTIN. Pp. ix + 301. (London: The McGraw-Hill Publishing Co., Ltd.) 15*s*.

The course of experimental work covered in this manual is of the standard of the intermediate examinations in the British universities. The book, which has reached its fourth edition, was primarily written for the students in the University of Wisconsin, so that many of the experimental details, such as resistances to be unplugged, apply to the apparatus set out for the use of the students in the Wisconsin laboratories. The directions given are comprehensive, some pertinent questions are appended to each description of an experiment and, except in the case of the simple experiments in light, the student is told what curves he should plot. The experiments on mirrors and lenses are not at all good. For example, in the case of the concave mirror when the lamp is inside the principal focus, "have some one hold a pencil behind the mirror and then determine, by moving the head from side to side, when the image and the pencil appear to be at the same distance on the other side of the mirror".

The scientist has always given his work freely to the world, so that it is with regret that one reads the announcement that "this book, or parts thereof, may not be reproduced in any form without permission of the publishers". Of necessity in books of this kind "old songs turn up again". Presumably, therefore, it is the publisher who has to realize the fact, which must be known by each of the authors, that

"When 'Omer smote 'is blooming lyre,
He'd 'eard men sing by land an' sea;
An' what 'e thought 'e might require,
'E went and took—the same as me."

J. H. B.

*The Rational Quartic Curve in Space of Three and Four Dimensions*, by H. G. TELLING. Pp. vi + 78. (Number 34 of the Cambridge Tracts. Cambridge: The University Press.) 5*s*.

The subject of higher geometry is one which, as far as one can foresee, is never likely to have a direct bearing on or application to physics. It is really a means of expressing algebraic theorems succinctly, and with perhaps less of rigour or generality than the analyst would require. The present tract enters on the four-dimensional problem without any preliminaries, and then proceeds to the problem in three dimensions. The book is packed exceedingly full of results, and is in fact a marvel of compression which could hardly be read profitably without a particularly good knowledge of the subject as set out in the general treatises. The standard of printing is all that we have come to expect in books from the Cambridge Press, and the book is cheaper than any of its last twelve predecessors in the series of which it forms part. May we dare to hope that this heralds a gradual fall in the prices of mathematical works?

J. H. A.

*The Scientist in Action*, by WILLIAM H. GEORGE, M.Sc., Ph.D., F.Inst.P. Pp. 355. (London: Williams and Norgate, Ltd.) 10*s*. 6*d*. net.

This book is the result of an attempt by the author to clarify his mind on the relationship between experiment and theory and more especially on the relationship between experimental and mathematical physics, and can be recommended to those who wish to do likewise. In it he develops the theory of patterning which is similar to that adopted by Mach, Kirchhoff, Karl Pearson and Hobson and is in contradistinction to the theory of absolute truth or inner reality of science at present more widely used, especially by popular writers on scientific subjects. The author rejects the idea that "Science is based on measurement" in favour of the idea that "Science is based on fact, or human judgement

of coincidence". He says: "Traditionally, experiment and theory have always been linked by philosophy. I now see them as linked only by the human research worker. Logically, science is found to be inseparable from scientists, but quite separate from philosophy." And again: "Whilst the traditional way is to regard the facts of science as something like the parts of a jig-saw puzzle which can be fitted together in one and only one way, I regard them rather as the tiny pieces of a mosaic, which can be fitted together in many ways. A new theory in an old subject is for me a new mosaic pattern made with the pieces taken from an older pattern. To speak of the one as being right, and the other as wrong, is then as inappropriate to the scientific as to the artistic mosaic." Thus Dr George has abandoned the traditional view and treats scientific research as a problem in human action and the scientist merely as part of the paraphernalia of research. According to him the application of these ideas "is a way of explaining things without using the idea of cause.... The things here explained... are effects without any causes of the agent type."

The author makes some significant statements about the cost of research and estimates that the minimum average cost nowadays of a research paper covering one year's work is of the order of £500. Would it not be an excellent thing to bring this to the attention of all research workers, in the hope of concentrating the limited funds available for research upon problems which seem most important and fundamental, instead of on those which happen to be most convenient and suitable for the award of a higher degree?!

Dr George's process of "clarifying his mind" is reflected in the varying style of the book. Some passages are difficult and heavy, whilst in others the haze has evidently cleared and given place to bright sunshine with a consequent clear, jovial and altogether delightful style. He evinces a wide and intimate knowledge of everyday affairs, be it the child in the nursery, the technique of ballet, the thoughts of the ordinary post-graduate student, or the difficulties of driving a car in traffic and conducting an interesting conversation at the same time, all of which he uses in his mosaic pattern.

Dr George is Royal Society Sorby Research Fellow and an Honorary Lecturer in Physics in the University of Sheffield.

H. R. L.

*Flame*, by O. C. de C. Ellis and W. A. Kirkby. Pp. vi + 106. (Methuen's Monographs on Chemical Subjects.) 3*s*.

The two authors are members of the staff of the Flame Section of the Safety in Mines Research Board and are therefore well qualified to deal with the topic under review. The book takes the reader from the familiar jet flame to the flame that spreads through a prepared mixture of mutually reactive gases, summarizing in a concise manner the theories that have been recently propounded to account for the observed phenomena. The reader wishing to have a rapid survey of recent advances in our knowledge of the aerodynamical and chemical aspects of combustion will find the little volume a mine of information.

E. G.

*Controlled Humidity in Industry*, by M. C. Marsh, M.A., Ph.D. Pp. 121 with 43 figures. (London: Charles Griffin.) 6*s*.

The object of this book is to review the question of the control of humidity under industrial conditions and the principles of the instruments and plants used for that purpose. The author deals in turn with the methods of measuring, of increasing and of decreasing humidity, air-conditioning plants, and automatic humidity-controls. The book should be of great service to manufacturers, engineers and research workers as it sets out what has been accomplished in the variation and control of humidity to meet the requirements of modern industrial processes. The author brings to bear on the subject an unbiassed mind and puts at the disposal of the public information gathered in the course of his work as member of the staff of the Wool Industries Research Association.

A feature of the book which deserves mention is the two appendices, one dealing with the application of factory and workshop acts to artificially humidified buildings and the other a list of manufacturers of plants and instruments for humidity-control.

E. G.

*Electrical Engineering in Radiology*, by L. G. H. Sarsfield. Pp. 284. (London: Chapman and Hall, Ltd.) 25*s*. net.

The X-ray tube has now taken its place as a tool for technicians. Mr Sarsfield, speaking as an engineer, claims that "It is possible to take pride in the appearance and place confidence in the operation of modern X-ray gear, and this could certainly not have been said in the old days of noisy, inefficient inductance coils...primitive switch gear, and inadequate wiring", when it was only found in rooms "littered with odd pieces of ill-assorted apparatus joined together with various wires, some up in the air and some on the floor". Most physicists will appreciate the point, and should they wish to learn how this tidiness and efficiency may be achieved, this book will supply the information required. It treats of those portions of electrical engineering which have been applied to the development of X-ray technique. It is clearly written and includes quite recent developments, and all physicists who are concerned with large-scale investigations involving the generation, transmission and control of high voltages will find the book very helpful. Such questions as the design of transformers, insulating supports, terminals, and rectifiers are discussed, and there is much valuable advice based on the author's wide experience of this subject gained in the Research Department at Woolwich.

L. H.

*The Penrose Annual Review of the Graphic Art*, vol. 38, 1936. Edited by R. B. Fishenden. (London: Percy Lund Humphries and Co. Ltd.) 10*s*.

The volume under review marks the beginning of a new series in which the size has been increased to 11 by 8 in. The printing and illustrations are a sheer joy and one regrets that our scientific literature cannot be adorned occasionally by such magnificent examples of the block-maker's art. The volume contains a large number of general articles covering the field. These range from "The psychology of English and German posters" to "Electrical dot etching".

In the article by Major J. J. Kruger on photogravure in South Africa and in particular the printing of postage stamps, we are informed that the design of the current postage stamps for the South African Government was completed in Holland and printed on machines manufactured in Germany. It is claimed that as the result of such enterprise South Africa is the first member of the British Commonwealth of Nations to embark on printing its postage stamps in gravure.

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# PROCEEDINGS AT THE MEETINGS OF THE PHYSICAL SOCIETY

## *SESSION* 1935–36

*Except where the contrary is stated the meetings were held at the Imperial College of Science and Technology, South Kensington, the President being in the Chair.*

*October* 4, 1935.

The President announced that the Council had elected the following to Student Membership of the Society: William Thomas Cowling, Eric P. George, Edgar Charles Woods.

The following presentations were acknowledged with thanks by the Joint Library Committee:

32 volumes of the *Philosophical Transactions of the Royal Society* (Series A) (1893–1914) by Miss M. D. Waller, B.Sc., F.Inst.P.

Volumes 76 and 77 (1934–5) of the *Transactions of the Institution of Naval Architects* by Dr Evelyn Shaw, C.V.O.

The following papers were read:

"Wood-water relationships, I. Molecular sorption and capillary retention of water by Sitka spruce wood," by W. W. Barkas, M.Sc.

"A combined hydrogen and helium liquefier," by B. V. Rollin.

"Temperature-rise in a material of which the thermal properties vary with temperature," by J. H. Awbery, B.A., B.Sc., F.Inst.P.

The following papers were read in title:

"The electron-oscillation characteristics of an experimental plane-electrode triode," by R. A. Chipman, B.Sc., M.Eng.

"The conductivity of an orifice in the end of a pipe," by A. E. Bate, M.Sc., Ph.D., F.Inst.P.

"Absorption spectra of the chlorides and oxychlorides of sulphur," by R. K. Asundi and R. Samuel.

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*October* 18, 1935.

The following were elected to the Fellowship of the Society: George Graham Harvey, Spencer Robert Humby, Michael Nelkon, Cecil Rowntree, John Berriman Smith.

The following presentation to the Joint Library was announced from the Chair as having been received since the last meeting; the thanks of the Society were accorded to the donor:

Lodge, Sir Oliver. *My Philosophy*. Presented by the author.

The following papers were read:

"The effect of phase-change on the cochlea," by H. Hartridge, M.D., F.R.S.

"The dependence of sensitivity of the selenium-sulphur rectifier photoelectric cell on the obliquity of the incident light, and a method of compensation therefor," by G. P. Barnard, B.Sc., A.Inst.P.

"Thermochemical properties of nitrous oxide," by T. Carlton-Sutton, M.A., M.Sc., F.Inst.P., H. R. Ambler, Ph.D., F.I.C. and G. Wyn Williams.

The following papers were read in title:

"The integration of the glare effects from a number of glare sources," by B. H. Crawford, M.Sc., A.Inst.P.

"The discrimination of the saturation of colours," by F. L. Warburton, A.R.C.S., D.I.C., M.Sc., A.Inst.P.

"Fine structure in the ($^2$D) series limit terms of the $I^+$ spectrum," by S. Tolansky, Ph.D., A.Inst.P.

"Absolute intensities in the spectrum of a low-pressure quartz mercury-vapour discharge burner," by A. J. Maddock, M.Sc., A.Inst.P.

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*November* 1, 1935.

Christopher Locke Cook was elected to the Fellowship of the Society.

The President announced that the Council had elected Ernest Wallace Voice to Student Membership.

The following presentations to the Joint Library were announced from the Chair as having been received since the last meeting; the thanks of the Society were accorded to the donors:

Lodge, Sir Oliver. *Beyond Physics.* Presented by the author.

Schofield, J. and Schofield, J. Colin. *The Finishing of Wool Goods.* Presented by the authors.

The following papers were read:

"Experiments on conducting laminae in periodic magnetic fields," by J. McGarva Bruckshaw, M.Sc., Ph.D.

"An optical calibration problem," by T. Smith, M.A., F.R.S.

"On the combination of observational data," by H. Levy, M.A., B.Sc. and J. C. Gascoigne.

The following papers were read in title:

"The effect of crystal-size on lattice-dimensions," by G. I. Finch, M.B.E., F.Inst.P. and S. Fordham.

"A determination of the specific heats of aqueous solutions of potassium chloride," by C. J. B. Clews, B.Sc., A.Inst.P.

"The thermomagnetic properties of nickel: II," by W. Band, M.Sc. and Y. K. Hsü, M.S.

"The longitudinal thermoelectric effect. VI: Mercury," by J. L. CH'EN, M.S. and W. BAND, M.Sc.

"Use of rotating parallel plane glass blocks for cinematography and projection with continuously moving films," by H. DENNIS TAYLOR.

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*November* 15, 1935.

The following were elected to Fellowship of the Society: Leonard V. Chilton, K. R. Hope Johnston, R. W. Minter, C. E. Wynn-Williams.

The following papers were read:

"Dust figures formed by an electric spark," by A. E. BATE, M.Sc., Ph.D., F.Inst.P.

"Absolute measurement of the viscosity of liquid tin," by A. J. LEWIS, B.Sc., Ph.D.

"The measurement of very low relative humidities," by A. SIMONS, B.Sc.

The following papers were read in title:

"On the current-distribution in a loop aerial," by DENIS TAYLOR, M.Sc.

"A negative-resistance oscillator," by N. L. YATES-FISH, M.A., D.Phil.

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*December* 6, 1935.

The following were elected to Fellowship of the Society: Frank Holliday, John Ashworth Ratcliffe.

The President announced that the Council had elected Kenneth Harold Waters to Student Membership of the Society.

The Thomas Young Oration was delivered by Prof. CHARLES FABRY of the Institut d'Optique, Paris, who took as his subject "La vision dans les instruments d'optique."

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*December* 20, 1935.

The meeting was held in the Physics Department, University College, Gower Street, London, W.C. 1 by invitation of the Provost and Prof. E. N. da C. Andrade.

The laboratories of the department were open for inspection by members of the Society and their guests from 3.30 p.m. to 5 p.m. Various demonstrations were on view, mostly connected with problems of sound and vibration.

Introductory remarks on the general work of the Laboratory were made by Prof. E. N. da C. Andrade.

The following papers were read:

"The determination of viscosity by the oscillation of a vessel enclosing a fluid" (Part I), by E. N. DA C. ANDRADE, D.Sc., Ph.D., F.Inst.P., F.R.S. and Y. S. CHIONG, M.Sc.

"The determination of viscosity by the oscillation of a vessel enclosing a fluid" (Part II), by E. N. da C. Andrade, D.Sc., Ph.D., F.Inst.P., F.R.S. and Leonard Rotherham, M.Sc.

"The determination of the viscosity of liquid gallium over an extended range of temperature," by K. E. Spells, Ph.D.

"The ripple method of measuring surface tension," by R. C. Brown, Ph.D.

"A method of measuring the amplitude and damping of ripples," by R. C. Brown, Ph.D.

"A method of investigating the Hall effect," by H. S. Hatfield, Ph.D., F.Inst.P.

"The smoke method of measuring supersonic velocities," by R. C. Parker, B.Sc.

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*January* 7, 8, 9, 1936.

The Twenty-sixth Annual Exhibition of Scientific Instruments and Apparatus was held at the Imperial College of Science and Technology.

The following discourses were delivered:

"Some instruments used in recording sound on film," by R. A. Bull, B.Sc. (Eng.), A.M.I.E.E., of the Western Electric Company, Ltd.

"Electrical measurements before 1886," by R. W. Paul, M.I.E.E., F.Inst.P.

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*January* 24, 1936.

The President announced that the Council had elected the following to Student Membership of the Society: Percy George Forsyth, D. A. Jones, G. V. Stupart and Vincent Salmon.

A presentation was made to Mr T. Banfield.

The prizes and certificates awarded for the seventh competition in Craftsmanship and Draughtsmanship were presented.

The Presidential Address, entitled "Some reminiscences of scientific workers of the past generation, and their surroundings," was delivered by the Right Hon. Lord Rayleigh, M.A., Sc.D., LL.D., F.Inst.P., F.R.S.

The following papers were read in title:

"Some thermal and electrical aspects of the design of converters for the hydrogenation process," by A. Blackie, M.A., F.Inst.P., C. W. Ockelford and C. M. Cawley, Ph.D., M.Sc., A.R.C.S., D.I.C.

"Regularities in the spectrum of Iodine IV," by S. G. Krishnamurty, M.A.

"The effect of dissolved air on the specific heat of water over the range 15° to 20° C.," by E. O. Hercus, D.Sc., F.Inst.P.

"The ranges of $\alpha$ particles in photographic emulsions," by H. J. Taylor, M.Sc. and V. D. Dabholkar, M.Sc.

"The behaviour of a single-hair hygrometer under varying conditions of temperature and humidity," by W. G. Iles and Kathleen Worsnop.

"The $\beta$-ray spectra of some induced radioactive elements resulting from neutron bombardment," by R. Naidu, D.Sc. and R. E. Siday, B.Sc.

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*February* 7, 1936.

The following were elected to the Fellowship of the Society: Henry Anthony Patrick Disney, Louis Essen, Frederick Hillman Gage, Ernest Pollard, Thomas Bertram Rymer, Henry Sessions Souttar, Alan George Gale Thomas, E. F. Forsyth, Herbert James Harold Starks (Transfer from Student Membership), Humphry Montague Smith (Transfer from Student Membership), Edward John Wenham (Transfer from Student Membership).

It was announced that the Council had elected Laurence William Walker to Student Membership of the Society.

The Thirteenth Duddell Medal was presented to Dr C. V. Drysdale, C.B., O.B.E., D.Sc., M.I.E.E., F.Inst.P.

The following papers were read:

"The viscosities of some liquid refrigerants," by J. H. Awbery, B.A., B.Sc., F.Inst.P. and Ezer Griffiths, D.Sc., F.Inst.P., F.R.S.

"The thermal and electrical conductivities of metals and alloys. Part II: Some heat-resistant alloys from 0° to 800° C.," by R. W. Powell, B.Sc.

The following papers were read in title:

"The properties of Heusler's alloy, and the true specific heat of manganese and its discontinuity," by J. R. Ashworth, D.Sc.

"The mechanical force on bodies of small susceptibility due to induced magnetization," by G. F. J. Temple, D.Sc., Ph.D., F.Inst.P.

A demonstration was given by Major C. E. S. Phillips, O.B.E., F.Inst.P., of "An experiment illustrating the support of a weight by a vertical tube containing sand, the lower end being covered by a thin membrane."

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*February* 21, 1936.

The following were elected to the Fellowship of the Society: Ladislaus Kecskemeti, Donald McMillan, John Thomas Miller, Geoffrey William Warren, Roy Ratcliffe Williams.

The following papers were read:

"On a new type of electronic oscillator tube with parallel plane grids," by W. A. Leyshon, Ph.D., F.Inst.P.

"An experimental investigation of the validity of Ohm's law for metals at high current-densities," by H. M. Barlow.

The following papers were read in title:

"The relation between the apparent intensity of a beam of light and the angle at which the beam strikes the retina," by W. D. WRIGHT, D.Sc., A.R.C.S., D.I.C. and J. H. NELSON, B.Sc., A.R.C.S.

"The use of thermocouples for psychrometric purposes," by R. W. POWELL, B.Sc.

"Thermal diffusion in deuterium mixtures," by K. E. GREW, Ph.D. and B. E. ATKINS, B.Sc.

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*March* 6, 1936.

The following were elected to the Fellowship of the Society: Alec Raymond Bennett, John Percival Vissing Madsen, Ronald Osmond Jenkins (Transfer from Student Membership).

The following papers were read:

"The accurate determination of ionospheric equivalent heights," by E. C. HALLIDAY, Ph.D.

"The variation of double refraction in celluloid with the amount of permanent stretch (*a*) at constant temperature, and (*b*) at different temperatures," by F. C. HARRIS, M.Sc., Ph.D. and B. R. SETH, M.A., Ph.D.

"A method of measuring secondary-electron emission from filaments," by L. R. G. TRELOAR, B.Sc.

The following papers were read in title:

"The thermal constants of setting concrete," by H. SHEARD, M.Sc.

"On the development of the quantum equation and a possible limit to its application," by H. T. FLINT, D.Sc.

"Note on the three absolute systems of electrical measurements," by the late Sir RICHARD GLAZEBROOK, F.R.S.

"The fourth unit of the Giorgi system of electrical units," by the late Sir RICHARD GLAZEBROOK, F.R.S.

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*March* 20, 1936.

*Annual General Meeting.*

The Minutes of the previous Annual General Meeting were read and accepted as correct.

The reports of the Council and Hon. Treasurer were adopted.

The Officers and the Council for 1936–7 and the Auditors were elected.

Votes of thanks were accorded to the retiring Officers and Council and to the Governors of the Imperial College of Science and Technology.

*Ordinary Meeting.*

Vernon Ellis Cosslet was elected to the Fellowship of the Society.

It was announced that the Council had elected the following to Student Membership of the Society: Frederick James Bryant, Anson Quinton, John Bernard Warren.

The following papers were read:

"The relation between rate and arc for a free pendulum," by E. C. Atkinson, M.A.

"On Airy's disturbance integrals and knife-edge supports for pendulums," by E. C. Atkinson, M.A.

The following papers were read in title:

"Intensities in the principal series of lithium," by S. Weintroub, M.A.

"Electron energies and excitation in the helium positive column," by F. Llewellyn Jones, M.A., D.Phil.

"On absorption spectra and photodissociation of some inorganic molecules," by M. Jan-Khan and R. Samuel.

The following demonstration was given by E. Tyler, D.Sc., F.Inst.P.: "Experiments with a new type of ripple tank."

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*April* 3, 1936.

Arthur Warmisham was elected to the Fellowship of the Society.

The following presentation to the Joint Library was announced as having been received since the last meeting; the thanks of the Society were accorded to the donor:

Boutry, G. A. *Les Phénomènes Photoélectriques et leurs Applications.* Presented by the author.

The following papers were read:

"Wood-water relationships, II. The fibre saturation point of beech wood," by W. W. Barkas, M.Sc.

"Time lag in photoelectric cells," by N. R. Campbell, Sc.D., F.Inst.P., H. R. Noble and L. G. Stoodley.

The following papers were read in title:

"Ultra-violet band systems of SiCl," by W. Jevons, D.Sc., Ph.D., F.Inst.P.

"The size of atmospheric nuclei: some deductions from measurements of the number of charged and uncharged nuclei at Kew Observatory," by H. L. Wright, M.A.

*May* 1, 1936.

It was announced that the Council had elected J. Stewart Marshall to Student Membership of the Society.

The following papers were read:

"The magnetic properties of manganese heated in nitrogen," by L. F. Bates, D.Sc., F.Inst.P., R. E. Gibbs, D.Sc., F.Inst.P. and D. V. Reddi Pantulu, M.Sc.

"Transmission of light through a pile of parallel plates," by R. A. Hull, B.A.

The following demonstrations were given:

"An experiment upon a method of increasing the rate of settlement of suspensions," by E. T. Wilkins, M.Sc., Ph.D.

"The method of assessing the extent and rate of settlement in suspensions and emulsions," by R. A. A. Taylor, A.R.C.S., M.Sc.

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*May* 15, 1936.

It was announced that the Council had elected M. Akhtar to Student Membership of the Society.

The Twenty-first Guthrie Lecture was delivered by Prof. F. A. Lindemann, M.A., Ph.D., F.R.S., of the University of Oxford, who took as his subject "Physical Ultimates."

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*June* 5, 1936.

The meeting was held in the Physics Department, Bedford College, Regent's Park, N.W. 1, by kind permission of Prof. W. Wilson. The Department was open from 3.30 p.m.

J. C. K. Rav was elected to the Fellowship of the Society.

The following papers were read:

"The mass of a convected field and Einstein's mass-energy law," by W. Wilson, D.Sc., Ph.D., F.R.S.

"Some experiments with neutrons having thermal energies," by J. R. Tillman, B.Sc., A.R.C.S.

"On the passage of neutrons through paraffin wax," by P. B. Moon, M.A., Ph.D.

"Notes on the neutrality of the neutron," by P. B. Moon, M.A., Ph.D.

The following papers were read in title:

"The structure of some metallic deposits on a copper single crystal as determined by electron-diffraction," by W. Cochrane, Ph.D.

"The effect of an acoustically absorbent lining upon the sound-insulating value of a double partition," by J. E. R. Constable, M.A., Ph.D.

"An improved numerical method of two-dimensional Fourier synthesis for crystals," by H. Lipson and C. A. Beevers.

*June* 19, 1936.

A visit to the Research Laboratories of the General Electric Company, Ltd. was arranged.

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*June* 26, 1936.

The following were elected to Fellowship of the Society: Jessie Cattermole, James Chadwick, John Charles Wilson.

The following papers were read:

"The determination of absolute units," by NORMAN CAMPBELL, Sc.D., F.Inst.P.

"The density and coefficient of expansion of liquid gallium over a wide range of temperature," by W. H. HOATHER, M.Sc., A.Inst.P.

The following papers were read in title:

"The phenomenon of 'spreading' in the first positive bands of $N_2$," by E. T. S. APPLEYARD.

"The paramagnetism of the rare-earth sulphates at low temperatures," by L. C. JACKSON, M.Sc., Ph.D., A.Inst.P.

"The magnetic properties of amalgams," by L. F. BATES, D.Sc., Ph.D., F.Inst.P. and L. C. TAI, B.Sc.

"The magnetic properties of chromium," by L. F. BATES, D.Sc., Ph.D., F.Inst.P. and A. BAQI, M.Sc.

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ADDENDUM to p. xvi of Vol. 47.

At the meeting held on June 21, 1935, the following were elected to the Fellowship of the Society: Gerald Thomas Prestoe Tarrant, Chintamani Ramchandra Dhodapkar, and Kwoh-Ting Li.

# REPORT OF COUNCIL FOR THE YEAR ENDING FEBRUARY 29, 1936

## MEETINGS

DURING the period under review 15 Ordinary Science Meetings were held at the Imperial College of Science and Technology. At these meetings 85 papers were presented, of which 56 were read in title. Seven demonstrations were given.

A Science Meeting was held on December 20, 1935, at University College by the kind invitation of the Provost and Professor E. N. da C. Andrade, F.R.S., and demonstrations were arranged in the laboratories of the Physics Department by the staff and research students.

The Thomas Young Oration was delivered by Professor Charles Fabry on December 6, the subject being "Vision in Optical Instruments."

The Presidential Address entitled "Some reminiscences of scientific workers of the past generation, and their surroundings" was delivered on January 24, 1936, by the Right Hon. Lord Rayleigh, M.A., Sc.D., D.Sc., LL.D., F.Inst.P., F.R.S.

## SUMMER MEETING AT THE ROYAL HOLLOWAY COLLEGE, ENGLEFIELD GREEN, SURREY

A visit was paid on May 25 to the Royal Holloway College by kind invitation of the Principal, Miss E. C. Higgins, and Professor F. Horton, F.R.S. The science laboratories were visited and also the picture gallery, where members and their guests were entertained to tea.

On the return journey Eton College was visited. The party was kindly received by the Vice-Provost, Mr C. H. K. Marten, who gave an account of the history of the School.

## THE DUDDELL MEDAL

The Twelfth Duddell Medal was presented to Dr W. Ewart Williams on March 15, 1935, for his work on interferometry.

The Council awarded the Thirteenth Duddell Medal to Dr C. V. Drysdale for his work in the design of electrical and optical instruments. The presentation was made on February 7, 1936.

## ANNUAL EXHIBITION

The Twenty-sixth Annual Exhibition was held on January 7, 8, and 9, 1936, at the Imperial College of Science and Technology, by the courtesy of the Governing Body.

There were 84 exhibitors in the Trade Section and 29 in the Research and Experimental Section. The attendance during the three days was over 8500.

The following discourses were given:

"Some Instruments used in the Recording of Sound on Film," by R. A. Bull, B.Sc. (Eng.), A.M.I.E.E.

"Electrical Measurements before 1886," by Robert W. Paul, M.I.E.E., F.Inst.P.

## PROGRESS REPORTS

The first volume of the series of Annual Progress Reports on Physics was issued in February, 1935, and a reprint was necessary.

The second volume is just published and is now on sale.

## THE *PROCEEDINGS*

The table following shows the remarkable rate of growth of the volumes of the *Proceedings* and the corresponding increase in the membership of the Society:

| Year | 1924–5 | 1925–6 | 1926–7 | 1927–8 | 1928–9 | 1929–30 | 1931 | 1932 | 1933 | 1934 | 1935 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Pages | 355 | 494 | 472 | 349 | 605 | 606 | 643 | 624 | 854 | 914 | 1154 |
| No. of Fellows | 651 | 678 | 691 | 700 | 735 | 765 | 782 | 910 | 945 | 944 | 962 |

It is obvious that such a growth involves the Society in seriously increased printing expenses and Fellows communicating papers are earnestly requested to see that the text of the papers is as brief as is consistent with clarity.

## REPRESENTATION ON OTHER BODIES

The three members who have represented the Physical Society on the British National Committee for Physics are Dr E. Griffiths, Mr T. Smith and Mr J. H. Awbery.

Professor Allan Ferguson, Dr D. Owen, Professor G. F. J. Temple and Mr J. H. Awbery have represented the Society on the Committee of Management of Science Abstracts.

Professor E. V. Appleton and Dr A. B. Wood have been representatives of the Society on the National Committee for Radio-Telegraphy.

Dr A. B. Wood and Dr E. Griffiths have represented the Society on the Board of the Institute of Physics.

## OBITUARY

The Council records with deep regret the deaths of the following Fellows: Mr J. G. Bower, Sir Richard Glazebrook, Mr J. P. Kirkman, Sir John McLennan, Dr D. K. Morris.

## MEMBERSHIP ROLL AT DEC. 31, 1935

| | Total Dec. 31, 1934 | Changes during 1935 | | | | Total Dec. 31, 1935 |
|---|---|---|---|---|---|---|
| *Honorary Fellows* | 11 | | | | – | 11 |
| *Honorary Fellows (Optical Society)* | 8 | Deceased ... ... | | | 1 | 7 |
| *Ex-officio Fellows* | 4 | | | | – | 4 |
| *Ordinary Fellows* | 921 | Elected ... ... | | 56 | | |
| | | Student transfers ... | | 4 | | |
| | | | | 60 | | |
| | | Deceased ... ... | 8 | | | |
| | | Resigned or lapsed ... | 33 | | | |
| | | | | 41 | | |
| | | Net change ... ... | | | 19 | 940 |
| *Students* | 67 | Elected ... ... | | 28 | | |
| | | Transferred to Fellow | 4 | | | |
| | | Resigned or lapsed ... | 7 | | | |
| | | | | 11 | | |
| | | Net increase ... ... | | | 17 | 84 |
| *Total Membership* | 1011 | Net increase ... ... | | | 35 | 1046 |

# REPORT OF THE HONORARY TREASURER

A moderate increase in the Society's income was outweighed by a large increase in the expenditure on its *Proceedings*, resulting in a deficit of £288. 8*s*. 1*d*. It is hoped that the steps which the Council is now taking will prevent such deficit next year. The publication of the first annual *Reports on Progress in Physics* has involved no charge on the general funds, the sum of £60 having been transferred from the special fund subscribed, to cover an estimated deficit.

The Society's investments have been valued at market prices on December 31st, 1935 through the courtesy of the Manager of the Charing Cross Branch of the Westminster Bank.

(Signed) ROBERT W. PAUL
*Honorary Treasurer*

*February 25th*, 1936

# INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED 31ST DECEMBER, 1935

| 1934 £ s. d. | Expenditure | £ s. d. | £ s. d. |
|---|---|---|---|
| | *To* | | |
| 534 0 0 | "Science Abstracts" | | 583 0 0 |
| | Normal Publications: | | |
| 1877 9 1 | "Proceedings" and Exhibition Catalogue | 2299 17 0 | |
| 56 9 0 | Notices | 39 19 11 | |
| 222 12 8 | Editorial and General Expenses | 204 5 11 | |
| | | | 2544 2 10 |
| | Special Publications: | | |
| | Printing and Dispatching "Reports on Progress in Physics" (Vol. I) | 574 2 7 | |
| | *Less* Transfer from Special Reports Fund | 60 0 0 | |
| | | 514 2 7 | |
| | Printing and Dispatching "Report of International Conference" | 634 11 5 | |
| 106 12 10 | *Printing and Dispatching "Report on Geometrical Optics"* | | |
| | | | 1148 14 0 |
| 272 16 9 | Postage on Normal Publications and General Correspondence | | 350 15 2 |
| 34 4 4 | Expenses at Meetings | | 32 19 0 |
| 10 12 6 | Periodicals and Library | | 8 18 11 |
| | Institute of Physics: | | |
| 500 0 0 | Clerical Assistance and Office Expenses | 528 16 0 | |
| 297 11 10 | Electricity, Labour and Sundries in connexion with Exhibition | 272 13 4 | |
| | | | 801 9 4 |
| 101 7 7 | Sundry Expenses | | 92 3 9 |
| | Honoraria to Lecturers | | 40 0 0 |
| | Printing and Dispatching List of Members | | 49 7 1 |
| 95 7 10 | *Balance, being excess of Income over Expenditure, carried forward to Accumulated Fund* | | |
| £4109 4 5 | | | £5651 10 1 |

| 1934 £ s. d. | Income | £ s. d. | £ s. d. |
|---|---|---|---|
| | *By* | | |
| | Subscriptions: | | |
| 27 6 0 | Entrance Fees | 56 14 0 | |
| 1659 5 7 | Fellows* | 1654 18 10 | |
| 4 4 0 | Voluntary† | 4 4 0 | |
| 32 5 6 | Students | 39 13 6 | |
| 106 14 6 | For "Science Abstracts" and Advance Proofs | 109 0 9 | |
| | | | 1864 11 1 |
| 71 10 0 | Transfer from Life Composition Fund of amounts paid by Fellows now deceased | | 30 0 0 |
| 825 14 6 | Sales: | | |
| | Normal Publications | 955 11 2 | |
| | Progress Reports (Vol. I) | 514 11 10 | |
| | International Conference Reports | 569 11 11 | |
| | | | 2039 14 11 |
| 374 14 6 | Advertisements in Publications | | 396 2 3 |
| | Dividends from Investments | 272 11 2 | |
| | *Less* Transfer to Duddell Memorial Fund | 14 0 0 | |
| 247 19 0 | | | 258 11 2 |
| 759 10 10 | Exhibitors' and other Payments on account of Exhibition | | 774 2 7 |
| | Balance, being excess of Expenditure over Income, carried forward to Accumulated Fund | | 288 8 1 |
| £4109 4 5 | | | £5651 10 1 |

* Eighty-seven Fellows paid reduced subscriptions by the arrangement with the Institute of Physics, the total rebate being £34. 13*s*. 6*d*.

† Voluntary subscriptions are subscriptions paid by Fellows who compounded for the low sum of £10.

## Liabilities

| | £ s. d. | £ s. d. | £ s. d. |
|---|---|---|---|
| *Sundry Creditors* | | 1623 19 2 | |
| *Life Compositions:* | | | |
| As on December 31, 1934 | 1571 10 0 | | |
| *Add* Payment during year | 21 0 0 | | |
| | 1592 10 0 | | |
| *Less* Transfer to Income and Expenditure Account of amounts paid by Fellows now deceased | 30 0 0 | | |
| | | 1562 10 0 | |
| *Subscriptions paid in advance:* | | | |
| Members | 35 13 2 | | |
| Publications | 256 5 0 | | |
| | | 291 18 2 | |
| | | | 3478 7 4 |
| **Trust Funds** | | | |
| *W. F. Stanley Trust Fund* | | 336 0 0 | |
| *Duddell Memorial Trust Fund* | | 446 12 9 | |
| *Optical Convention* 1926, *Trust Account* | | 39 11 9 | |
| *"Special Reports on Physics" Account* | | 83 1 0 | |
| | | | 905 5 6 |
| **General Fund** | | | |
| *Accumulated Fund:* | | | |
| As on December 31, 1934 | | 4071 8 10 | |
| *Less* Decrease in value of Investments | 237 0 0 | | |
| *Less* Balance of Income and Expenditure Account | 288 8 1 | | |
| | | 525 8 1 | |
| | | 3546 0 9 | |
| *A. W. Scott Bequest* | | 185 19 6 | |
| | | | 3732 0 3 |
| | | | £8115 13 1 |

## Assets

| | £ s. d. | £ s. d. | £ s. d. |
|---|---|---|---|
| *Investments at Market Value on December* 31, 1935: | | | |
| £1500 Consolidated Stock 2½% | 1301 0 0 | | |
| £1000 War Loan 3½% Inscribed "A" Account | 1060 0 0 | | |
| £750 War Loan 3½% Registered | 795 0 0 | | |
| £650 Funding Loan 4% | 760 0 0 | | |
| £500 India 3½% Stock | 485 0 0 | | |
| £400 Lancaster Corporation 3% | 392 0 0 | | |
| £399 London Midland and Scottish Railway 4% Debenture Stock | 427 0 0 | | |
| £1000 London Midland and Scottish Railway 4% Preference Stock | 820 0 0 | | |
| £500 London and North Eastern Railway 4% Debenture Stock | 530 0 0 | | |
| £150 Southern Railway 5% Debenture Stock | 198 0 0 | | |
| £300 Southern Railway Preferred Ordinary Stock | 246 0 0 | | |
| £442 Southern Railway Deferred Ordinary Stock | 90 0 0 | | |
| £400 War Loan 3½% Inscribed "B" Account | 424 0 0 | | |
| | | 7528 0 0 | |
| *Dividends due from Investments* | | 35 4 9 | |
| *Inland Revenue—Income Tax claimed for* 1935 | | 17 13 2 | |
| *Subscriptions due* | | 26 12 0 | |
| *Sundry Debtors* | | 149 5 6 | |
| *Cash at Bank: Current* | 347 2 6 | | |
| *Cash in Hand* | 11 15 2 | | |
| | | 358 17 8 | |
| | | £8115 13 1 | |

*Stock of publications on* 31*st December*, 1935. *At Honorary Treasurer's valuation* . . . . £1627 18 7

ROBERT W. PAUL, *Honorary Treasurer.*

We have audited the above Balance Sheet and have obtained all the information and explanations we have required. We have verified the Bank Balance and the Investments. In our opinion such Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given to us and as shown by the books of the Society.

Spencer House, South Place, E.C. 2
25*th February*, 1936.

KNOX, CROPPER & CO.,
*Chartered Accountants.*

## LIFE COMPOSITION FUND ON DECEMBER 31ST, 1935

| | £ | s. | d. |
|---|---|---|---|
| 52 Fellows paid £10 | 520 | 0 | 0 |
| 1 Fellow paid £15 | 15 | 0 | 0 |
| 16 Fellows paid £21 | 336 | 0 | 0 |
| 1 Fellow paid £30 | 30 | 0 | 0 |
| 21 Fellows paid £31. 10s. | 661 | 10 | 0 |
| | £1562 | 10 | 0 |

## W. F. STANLEY TRUST FUND

| | £ | s. | d. | | £ | s. |
|---|---|---|---|---|---|---|
| Carried to Balance Sheet | 336 | 0 | 0 | £300 Southern Railway Preferred Ordinary Stock | 246 | 0 |
| | | | | £442 Southern Railway Deferred Ordinary Stock | 90 | 0 |
| | £336 | 0 | 0 | | £336 | 0 |

## DUDDELL MEMORIAL TRUST FUND

### CAPITAL

| | £ | s. | d. | | £ | s. |
|---|---|---|---|---|---|---|
| Carried to Balance Sheet | 424 | 0 | 0 | £400 War Loan 3½% Inscribed "B" Account | 424 | 0 |

### REVENUE

| | £ | s. | d. | | £ | s. |
|---|---|---|---|---|---|---|
| Honorarium to Medallist | 15 | 0 | 0 | Balance on December 31st, 1934 | 23 | 12 |
| Balance carried to Balance Sheet | 22 | 12 | 9 | Interest | 14 | 0 |
| | £37 | 12 | 9 | | £37 | 12 |

## OPTICAL CONVENTION, 1926, TRUST ACCOUNT

| | £ | s. | d. | | £ | s. |
|---|---|---|---|---|---|---|
| Balance carried to Balance Sheet | 39 | 11 | 9 | Balance on December 31st, 1934 | 37 | 0 |
| | | | | Sales of Publications | 2 | 11 |
| | £39 | 11 | 9 | | £39 | 11 |

## A. W. SCOTT BEQUEST

| | £ | s. | d. | | £ | s. |
|---|---|---|---|---|---|---|
| Expenses in connexion with Library | 14 | 0 | 0 | Balance on December 31st, 1934 | 199 | 19 |
| Balance carried to Balance Sheet | 185 | 19 | 6 | | | |
| | £199 | 19 | 6 | | £199 | 19 |

## "SPECIAL REPORTS ON PHYSICS" ACCOUNT

| | £ | s. | d. | | £ | s. |
|---|---|---|---|---|---|---|
| Transfer to Income and Expenditure Account of estimated deficit on Progress Reports (Vol. I) | 60 | 0 | 0 | Balance on December 31st, 1934 | 142 | 10 |
| | | | | Donations during 1935 | | 11 |
| Balance carried to Balance Sheet | 83 | 1 | 0 | | | |
| | £143 | 1 | 0 | | £143 | 1 |

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